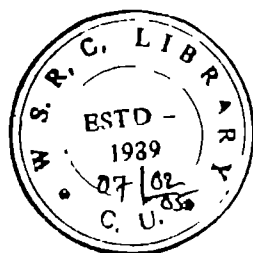


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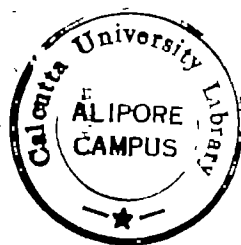
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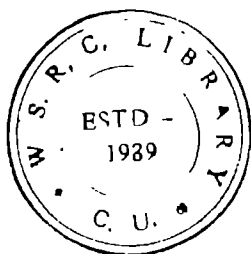


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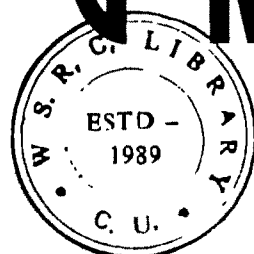
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Gender and Science: New Issues

The splendid essays in this issue, all on the topic of science, arrived independently in the journal office, and we then commissioned the interview, review essay, and book reviews. Gender and science remains a perennially fascinating topic for several reasons: scientific institutions are among those that continue to discriminate against women most; the purported neutrality of science makes it appear resistant to gender perspectives; and cultures around the globe are increasingly permeated by modern Western scientific institutions, practices, and philosophies that disseminate ideological assumptions and discrimination. Gender studies of science draw on the larger fields of science studies and, of course, feminist studies. Increasingly they are learning to draw on antiracist and anti-imperial studies as well. The essays here demonstrate the increasing analytic sophistication that this conjunction of resources has created, and they highlight the work that lies ahead for researchers, scholars, and activists interested in these topics. We hope you enjoy these analyses and discussions as much as we have.

—The Editors

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Signs Special Issue: Dilemmas In Feminist Social Research

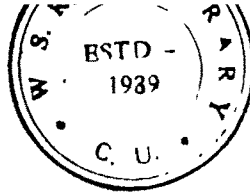
S*igns: Journal of Women in Culture and Society* seeks submissions for a special issue on "Dilemmas in Feminist Social Research," slated for publication in summer 2005. For three decades, feminists in the natural and social sciences have questioned not only the care with which research in history and the social sciences has been conducted but also the adequacy of standard methods to achieve their claimed goals. They have challenged the desirability of those goals and the philosophies of science that have prescribed them. They have introduced alternative research designs and theories of good method. Today many women's studies curricula require courses in research methods appropriate to feminist projects.

Yet thirty years of such work have not made feminist methods and methodological choices uncontroversial. Virtually all of the puzzling and troubling issues of feminist theory reappear in the context of methods. We solicit essays that address these controversies and, especially, that report how researchers' own liberatory methodological plans have had unexpected results intellectually or politically. How should the researcher position herself and her subjects in her research projects? What notions of the subjectivity of the researcher and of the researched are appropriate for feminist projects? How can one effectively bring the intersectionality of gender, race, class, sexuality, and culture to a research project? Can social research ever escape the "colonial model"? In which contexts can exposing the researcher's personal situation, experiences, voice, and emotions appropriately advance a research project, and in which contexts not? What limits should feminists impose on projects that "study down"? Should the standard restrictions on therapists' relations with their clients (not always honored, of course) hold for social researchers and their research subjects? Are some methods more liberatory than others, and wherein does such potential lie? What are the distinctive feminist issues about methods in history, sociology, political science, economics, anthropology, and other fields of social research? Why are methods *not* an issue in some feminist research areas?

The special issue editors are Sandra Harding (Graduate School of Education and Information Studies, UCLA) and Kathryn Norberg (history,

UCLA). Please submit articles (three copies) not later than October 31, 2003, to *Signs*, "Dilemmas in Feminist Social Research," University of California, Los Angeles, 1400H Public Policy Building, Los Angeles, CA 90095-7122.

Please observe the guidelines printed at the back of the most recent issues of the journal or at <http://www.journals.uchicago.edu/Signs/instruct.html>.



Sexing the Hyena: Intraspecies Readings of the Female Phallus

The only way to "sex" most mammals at birth is to inspect their genitals; with humans, it is the first thing we do, and all kinds of trouble follows if there is any doubt about the classification.¹ Thereafter, until male and female maturational pathways diverge, we rely, in the case of animals, on other identificatory markings to which the primary perception of sex is always attached. In the case of humans, infants are allocated names that identify them as individuals, but they are also dressed differentially: while the given name may be neutral, the clothing or ornament will not be; gender is available as a culturally specific visual signal.² Clothing is thus used to denote a given, to reassert the existence of a natural sexual difference that the clothing covers but claims to reveal. How this system works in practice clearly demonstrates that maleness is the norm from which females deviate: in contemporary North America, a baby will generally be identified as male by default unless s/he is very clearly marked as female, either by narrowly sex-specific clothing (a dress) or by a still narrower color coding (all items of apparel in pink).³

I received helpful comments from the three *Signs* reviewers of this article. In response to a question raised by one of them, I would like to make it clear that I have not visited the Berkeley Hyena Project in any capacity, nor have I interviewed any of the researchers working there. All material quoted in this article is taken from published research, interviews, and journalism.

¹ The literature on intersexes, both describing social and surgical interventions designed to resolve ambiguously sexed cases and protesting such interventions, is now extensive. See, e.g., Kessler 1990, 1998; Donohoe, Powell, and Lee 1991; Diamond and Sigmundson 1997; Preves 2002.

² While there are still some non-Western societies in which infants are not immediately named, the signaling of infant gender is now a feature of almost all cultures.

³ This observation is based only on anecdotal evidence; while there are several studies that examine how adults relate differently to infants that they assume, based on clothing cues, to be male or female, I am not aware of any that address how the original gender identification is made. A current example of male normativity is provided by the *Raggyrats'* apparently identical boy and girl twins, Phil and Lil. They are drawn the same, and they

Presumably, it is the perceived importance of this differentiation, at an age when other cues such as hair length or behavioral affect cannot be relied on, rather than its difficulty, that drives the extreme bifurcation of current North American infant dress. It might seem paradoxical that baby wear is more urgently gender dichotomized at a time when innate explanations for sexual differentiation have gained ascendancy over environmental ones—when, in other words, any amount of cultural intervention will, it is understood, still leave boys being boys. But it could also be the case that both clothing fashions and scientific orthodoxies about how bodies and brains acquire “sex” are expressions of gender anxiety, separate if contemporaneous responses to the present incoherence of discourses of female possibility.

My discussion of how gender is produced will here take the form of an account not of humans but of hyenas—of spotted hyenas in particular, which cause productive trouble for a gendering system based on visible difference because both males and females appear to have a penis. By attempting a genealogy of the hyena, focused on the female hyena body as “a body totally imprinted by history” (Foucault 1977, 148), I hope by means of an animal whose association with aberration (and, I shall argue, with specifically female aberration) seems dissoluble to raise questions about the compatibility of femaleness and normativity.

Disney’s *The Lion King* (1994) is much less obviously interested in gender than other productions from the same source. The reach of its narrative is broad, suavely reinvigorating hierarchy, patriarchy, “family values,” and heteronormativity by way of a few rollicking tunes and some supposedly unmediated animal behavior. But it is nonetheless relevant as a starting point for my purposes because of how this popular, globally available film imagines deviance: the villains of the piece take the form of evil, queer Uncle Scar (Jeremy Irons) and his deceitful, bumbling side-kicks, the hyenas. The portrayal of the hyenas draws on ancient traditions in depicting them as at once violent, greedy, devious, unprincipled, and hopelessly stupid, qualities encapsulated in the scene where Ed (Jim Cummings) chews on his own leg, mistaking it for a tasty scavenged meal. The refraction of this depiction through the lens of race is the only (nasty) new element in what is otherwise a familiar story.

Stephen Glickman, an authority in the field of hyena study, notes ruefully that the filmmakers should have known better, since they had availed themselves of the University of California’s captive colony of hyenas as

wear the same clothes. But while Phil is unmarked, Lal is the site of differentiation: her non-Philiness is established by the pink bow on the top of her head.

models for their drawing and characterization (1995, 504). Glickman, who describes himself as hyena "positive," having "bonded" with hyenas as the result of his African fieldwork (1995, 531), hopes and believes that contemporary scientific study of hyenas will lift the veil of ignorance surrounding them, dispelling centuries of misinformation and prejudice.⁴ It is the argument of this article, however, that the new methodologies and sites of hyena study, of which the University of California colony and the papers it has generated are a prime example, produce knowledges that reiterate rather than transform old ideas and patterns of representation. The hyena has always operated culturally as a sign of disorder, and I will demonstrate here that scientific examination of fetal androgen levels in the captive colony, for example, operates in the same way, discursively, as did medieval descriptions of the hyena as dangerous sex-changer; far from representing a counterblast to tradition, the new science, almost despite itself, deploys the hyena as a warning of the consequences of deviation from sex/gender norms. If there has been a discursive shift it has been in the form that knowledge takes: the hyena has moved from being the dangerous, unknowable other into the position of that which can be known, studied, and dissected but that is still other.

Apart from illustrating the continuity running beneath an apparent epistemological break, the hyena as representation is also instructive because of its function as focal point for the negotiation of gender anxiety. While both ancient and modern texts depict the hyena as sexually aberrant in a variety of ways, it comes increasingly in modernity to figure a specifically female deviance from gender norms. Contemporary scientific study, which focuses on the hormonal "masculinization" of female hyenas, represents a kind of *mise en abyme* of this process; gender abnormality, of which the hyena, now that she can be studied in the laboratory, is allegedly the exemplar, remains either directly or by association linked with a range of other variously antisocial attributes. The hyena comes both to reference and to enlarge on the potential horrors of stepping across the supposed boundaries of gender. Tropes whereby the hyena policed the limits of gender possibility are reinvigorated by their translation into science, the authoritative signifying system of modernity. Gender aberrance also becomes a figure for all other forms of deviance, making staying within these

⁴ Despite his apparent occupation in this article of the objectivist position that his scientific truth will be able to refigure the hyena once and for all, Glickman also comments on the frequency with which culturally useful elements of hyena lore—that they are scavengers, e.g.—persist as truth despite authoritative scientific denials (1995, 508).

bounds, and the consequences of stepping outside them, a terrifyingly all-or-nothing proposition: to transgress gender is to transgress everywhere.

Finally, I am seeking here to illuminate the ideological processes whereby certain ideas come to have and to retain truth value while others have no power to generate knowledge. The range of meanings that can attach to the hyena is curiously consistent, which is one of the reasons why Glickman, seeking to promote a sympathetic, nuanced account, so often nonetheless participates as a research scientist in a discourse that reinforces the hyena's identification with aberration. While this mechanism is a problem for the hyena, should we wish to reimagine her cultural role, it is more generally a concern for anyone seeking to widen the available terms of discussion about the formation of human sexuality and gender identity. But it is not an inevitable mechanism: the solidity of the hyena's attachment to deviance can be unsettled, even as it is revealed, if what is seen is visible as human and normative.

The historical hyena

Premodern representations of the hyena came to the West originally from three main sources: Aristotle, Pliny, and the *Physiologus*; ideas in the latter became the basis for the hyena that recurs in medieval bestiaries, other Christian texts, and, gradually, the metaphor of the everyday. Aristotle, focused on observing and categorizing the natural, and basing his argument on detailed anatomical description, denies that the hyena is either a hermaphrodite (having a set of both female and male genitalia) or an annual sex-changer (being "mounted" or "mounting" for sexual purposes in alternate years).⁶ Aristotle's position as empiricist refuting others' extravagant claims, as well as some of the magical attributes he continues to attribute to hyenas, indicate that the hyena's figuring of sex/gender ambiguity and of aberrant behavior generally have wide-ranging cultural sources. One site for the generation of this complex of ideas is suggested by Suzanne Pinckney Stetkevych, who notes that in pre-Islamic Arabic poetry hyenas are depicted as indulging in a raft of transgressive behaviors, including not only wholesale eating of those toward whom humans usually recognize social obligations (hosts or fallen enemies, e.g.) but also taking advantage of the engorged penises of the warrior dead for sexual pleasure, laughing and menstruating the while—before devouring the corpse. The list of tribal taboos the hyena breaks (against the eating of unsacrificed

⁶ Extended references to hyenas occur in Aristotle's *History of Animals* and *Generation of Animals* (1958).

meat, against unsanctioned sex, female immodesty, etc.) suggests a particular mechanism of cultural threat that the hyena appears to represent: "Where Arab tribal society recognizes a distinction, the hyena, as they perceive it, exhibits a confusion or inversion of categories" (1996, 26). Stetkevych also hypothesizes a kind of cumulative process to explain the hyena's extreme position in representation: "The hyena seems to have been understood as the ultimate transgressor, as one abomination seems to have generated another" (1996, 26).⁶ Both these properties, the tendency to provoke or emblemize an actual or potential category crisis and the folding of one form or area of deviance into another to the point where one transgression references all others, will turn out to be remarkably portable across cultures.

It is likely that tribal conceptions were, however culturally mediated, based on the existence of large hyena populations locally, as were Aristotle's observations. African sources, again with the benefit of an actual population, produce a bifurcated view of the hyena as at once profoundly risible and profoundly dangerous. Alma Gottlieb (1989) suggests that this contradictory combination of attributes and attitudes arises because, while the hyena is consistently seen as antisocial and subversive, in certain contexts and forms of narrative such potential violation of boundaries (because of extremes of greed, e.g.) is a matter for the hearer's identification, while in others it must be repudiated as a serious threat to the official social order.⁷

⁶ Stetkevych provides a table comparing hyena habits with tribal organization and taboo in a range of areas (including food, gender, the relation between sex and food, and hospitality) that illustrates the difficulty of disarticulating the "natural" hyena from the cultural perception. For example, hyenas eat carrion, while the tribe eats only sacrificed meat, so the hyena in the wild observably violates a human rule, and the difficulty of distinguishing between male and female hyenas violates tribal expectations that genders be both visually and behaviorally distinct. But the table juxtaposes to these categories those of sex/food, menstruation, and hospitality. In these instances the hyena's taboo violation is in the direction of extreme category confusion, according to tribal ideas: it allegedly has sex with corpses, has sex during menstruation, and is liable as a guest to eat its host. But this juxtaposition does not address the question of how the slippage occurs between hyena as animal and hyena as transgressive operator in human systems.

⁷ While several commentators remark on the contempt with which many different African peoples regard the hyena, citing its ugliness, stupidity, and scavenging habits, Alma Gottlieb also emphasizes the hyena's capacity to pollute social space by its presence; she cites, e.g., a Beng ritual that requires that a village be abandoned if a hyena is found to have defecated within its bounds (1989, 493-94). The threat represented by excrement is a threat from without, to life from death, whereas the specifically female threat from menstrual blood, e.g., is a threat from within: the hyena seems economically to encapsulate both (Kristeva 1982, 71).

The *Physiologus* lodges the sex-changing claim in the Western lexicon of hyenic meaning. In this text, thought to have been originally assembled from a variety of folk sources in Egypt around 200–400 C.E., subsequently translated from Greek and Latin into many vernacular languages, and widely available in the West, the natural world is seen primarily as a source of knowledge of the divine through earthly correspondences and as a source of moral instruction.⁸ Here the hyena is “an alternating male-female . . . unclean because it has two natures,” and this shifty doubling is the ground for a homily about inconstancy and men who become like women in difficult, ethically challenging circumstances (Curley 1979, 52–53). In Christian commentary thereafter, the hyena is firmly associated with both sexual excess and sexual aberration.⁹

Many of the elements that recur in medieval bestiaries—deviousness, grave robbing, and shape changing—are based on Aristotle, as well as on the more mythifying Pliny, and bring out other sinister, nonsexual transgressions with which the hyena is associated: “The hyena lives in the graves of dead men and feeds on their bodies . . . it follows the shepherds when they move their sheepfolds, and creeps round men’s houses at night. By dint of constant listening, it learns to call out and can imitate the human voice, so that it cunningly lures men outside and falls on them at night.

⁸ John Borwell, along with other sources, emphasizes the importance and wide distribution of the *Physiologus*: “Available in every Romance language as ‘the bestiary,’ it served as a manual of piety, a primer of zoology, and a form of entertainment” (1980, 141–42).

⁹ The third-century Christian commentator Clement of Alexandria illustrates the ways in which the hyena figures multiple forms of decadence and perversion simultaneously: “Since this animal is extremely lewd, it has grown under its tail in front of the passage for excrement a certain fleshy appendage, in form very like the female genitalia. This design of the flesh has no passage . . . leading to any useful part, I say, either to the womb or the rectum. It has, rather, only a great cavity, whence it devises its fruitless lust. . . . This same thing occurs in the case of both the male hyena and the female, because of their exceptional passivity. The males mount each other. . . . Only in the case of the hyena has nature devised this superfluous part for their excessive copulations, and it is consequently hollow, up to a point, for the use of the libidinous parts; but for the same reason the hollow is a blind alley” (translation of Clement of Alexandria’s *Protrepticus*, in Borwell 1980, 356–57). Clement’s doctrinal focus is on the use of an animal analogy to illustrate human failures to measure up to divine order, with fruitless lust expending itself in blind alleys a trope of benighted ignorance. But it is again notable that category confusion, expressed in terms of both sexual activity and the gendering of that activity, is the hyena’s especial *sin*. Provided with an extra appendage that is at the same time hollow—as if the hyena aspires simultaneously to penetrate and be penetrated at the site of an organ that is both phallic and anal—male and female switch and blur in a shimmer of undifferentiated excess.

It counterfeits human vomit and makes sounds like a man being sick to lure out dogs, so that it can devour them" (Barber 1992, 45–47).¹⁰

The medieval hyena is not the only beast that inhabits that vast wild area beyond the pale, which it represents and from which it emerges to threaten civilization, using human gullibility and frailty to ensnare its prey, but it is arguably the most extreme and the least redeemed by positive qualities.

From this rich and varied array of gruesome attributes, the hyena in modern Western representation evolves into a fairly predictable beast. Popular culture focuses on a few key qualities: greed, treachery, violence. Hence *Newsweek*: "Tummies full, teeth glistening, Bob Dole and Newt Gingrich preened last week, proud to be hyenas. But this time they may have bitten off more donkey flesh than they can chew. . . . Killing a bill that cracked down on lobbyists was simple, spiteful, self-interested obstruction" (Alter 1994, 41).

The idea of the hyena as a human impersonator, throwing its voice to snare the unwary, fades in its literal form after the seventeenth century, but the idea that the boundary between hyena and human just might be permeable, and with consequences more unpleasant than attend the discovery of other beasts within, does not seem to; this is the hyena that Horace Walpole invokes to suggest that the worst of animality has surfaced in the participants in the French Revolution, and it lies behind Charlotte Brontë's depiction of Bertha Mason in *Jane Eyre* as a "clothed hyena" that "rose up and stood tall on its hind feet" ([1847] 1987, 248).¹¹

In *Esquire*, the hyena appears as a lazy sexual predator, happy to steal his brother's wife (scavenged meat, no hunting required; Leyner 1996).¹²

¹⁰ Pliny is the immediate source for much of the lore on hyenas as expert ventriloquists of human speech; his *Natural History* (1958) also gives copious details about the magic properties of various parts of the hyena body.

¹¹ Walpole's references to hyenas in his letters of the 1790s often compare them favorably to the French, who are even more depraved than this most bloodthirsty of beasts: "On the French I cannot speak. . . . If all Mr Bruce's hyenas had met in three National Assemblies, they could not have produced similar horrors, for hyenas tear both men and women to pieces at once, but do not torture and keep them in constant alarms for three years together—they do not butcher hundreds and thousands more than they can devour. They do not terrify men to flight, and then persecute the wives and daughters of those they have terrified. Hyenas do not promise bribes to tigers to massacre men of certain descriptions, viz. kings, when tigers are neither hungry nor provoked—no, Sir, hyenas are not French philosophers" (Walpole 1937–83, 15:235–36).

¹² Mark Leyner's column is a subversive riff on Disney that employs old hyena representations with antic glee: "Why 'The Hyena King'? The lion expends tremendous time and energy grimly chasing his prey. The hyena, laughing maniacally, feasts effortlessly on prekilled

The hyena as sign of sexual ambiguity and aberration is as consistently available in the twentieth century as in the third. John Boswell's account of the medieval hyena suggests that sexual aberration is, for Christian commentary, its key attribute, and also that what is especially to be anathematized in these texts by means of the hyena is male homosexuality. As he points out, Clement of Alexandria's description of the hyena's extra orifice concludes by denouncing anal intercourse and sex between males (1980, 137–43). In Boswell's reading, sex changeability is metaphorical rather than actual, the passive recipient becoming feminized rather than female.¹³ This seems plausible as far as the early Church fathers and their doctrinal successors are concerned, although hyenas are also routinely used to point up the sins of adultery and general sexual excess. But (negative) feminine attributes are often also reattached to biological sex at the same moment as they serve to separate manly from unmanly males, and later references tend to support the proposition that at some point the hyena, not entirely inappropriately, does a sex change and becomes, typically, female. So Lyly's 1578 account of the ventriloquist hyena feminizes the pronoun: "When she speaketh like a man deviseth most mischief" (Lyly [1578] 1916, 97); Milton's Samson denounces Delila: "Out, out, hyaena! these are thy wonted arts, / And arts of every woman false like thee, / To break all faith, all vows, deceive, betray" (Milton [1671] 1971, 748).¹⁴ And Walpole's notorious epithet for Mary Wollstonecraft, "that hyena in petticoats," works both covertly and overtly around expectations of femininity transgressed.¹⁵

carion. Let your brother play the lion and spend weeks, months, perhaps even years pursuing and courting his wife. You be the hyena and tryst insouciantly with her against a cigarette machine in the bowels of some catering hall at a family reunion" (1996, 164).

¹³ Since Boswell's focus is on the process whereby male homosexuality is anathematized, he pays minimal attention either to the question of what female hyenas in particular might be imagined to be doing or to the ways in which sex changing might be, or result in, aberrations other than male homosexuality.

¹⁴ There is also evidence for a consistent identification of hyenas with women: see Gordon 1980 for a discussion of the links made between hyenas and witches in a range of ancient authorities.

¹⁵ Writing to Hannah More, a conservative champion of womanly subjection, on the occasion of her having proposed writing church tracts, Walpole pronounces: "Thou excellent woman! Thou reverse of that hyena in petticoats Mrs Woolstencroft, who to this day discharges her ink and gall on Mary Antoinette" (Walpole 1937–83, 31:397). Aside from the sexual suggestiveness of foul discharges, the petticoat works both to conjure inappropriate female usurpation of power (petticoat government), to produce the effect of liminality, of a hyena that is only partly, or aspirationally, human, to suggest a state of libidinous undress suitable for the discharging of gall, and to figure Wollstonecraft as somehow not just a hyena

The scientific hyena

Twentieth-century Western representations, when they aspire beyond the recycling of available metaphor, characteristically found their revision and reiteration of hyenic meaning upon direct or indirect observation of the hyena in the African wild.¹⁶ The remarkable freedom and power that marks preempiricist usage of the hyena—her capacity economically to denote manifold deviance—would seem likely to be curtailed by an increased attention to and deference for the conventions of science and fieldwork practice.¹⁷ Science comes late to the hyena, however, both as object of observation in the field and as laboratory subject; the initial modern perspective is that of the white hunter. We might understand Theodore Roosevelt and Ernest Hemingway as transitional figures in a gradual resituating of the hyena in the context of modern scientific rationalism inasmuch as, while they both base their renditions of the hyena on their experiences as hunters, the animal they see is brimful with human attributes, quite as unself-consciously an anthropomorphized tool as the hyena in the drawing room of an eighteenth-century house that has been visited by Wollstonecraftian ideas.¹⁸ The new old world of the bush becomes a recolonizable space for expansive Western visions of self in relation to a freshly available nature. The key binaries employed by white hunters as linguistic and cultural invaders on the savanna are bravery and cowardice, constructions effortlessly projected onto the available animal field, and only secondarily, and by way of emphasizing the primacy of white encounters with this demarcation system, onto the indigenous human population.

The hyena does not fit neatly into the taxonomy of the world as safari

but a hyena in drag, both actually female and a female who must masquerade as such for lack of femininity's key ingredient.

¹⁶ The hyena's position as "oriental" other to the occidental self remains constant; what is marked here is not a shift in that fundamental binary but in the nature of its investigation.

¹⁷ While versions of hyena "natural history" appeared during the sixteenth and seventeenth centuries, these tended not to be based on observation of hyenas in the field, even if they were by then primarily zoological exercises rather than theological ones. By the late eighteenth century, travelogues such as James Bruce's *Travels to Discover the Source of the Nile* (1790) had begun to bring back less mythic firsthand accounts to the West. The Count de Buffon's *The Natural History of Quadrupeds* (1830) represents an early flowering of the nineteenth-century taxonomic impulse; hyenas were first subjected to systematic observation and dissection of a laboratory kind at the end of the century by M. Watson, who published "On the Female Generative Organs of Hyena Crocuta" and "On the Male Generative Organs of Hyena Crocuta" in the *Proceedings of the Zoological Society of London* in 1877 and 1878, respectively. Watson's studies remained the basis for scientific investigation of the hyena until the inception of the Berkeley Hyena Project (BHP) in 1984.

¹⁸ See, e.g., Roosevelt and Heller 1915; and Hemingway 1935.

in which one may be either noble predator (the lion, paradigmatically) or long-suffering prey (any ungulate will do here). Since the available identities in this signifying system are hunter and hunted, when a human takes on the hunter position animals of both categories can be subsumed into another, that of game: good game plays the game of aggrandizing the dominant partner either by being a worthy adversary because dangerous (and preferably also beautiful and hard to catch) or by being a fine specimen of its essentially hunted self (beautiful, yet marked by masculinity if possible [horns], hard to find and difficult to catch). Hyenas make unsatisfactory participants in the theoretically infinitely flexible category of the hunted, as is evident in Hemingway's account of shooting one, presented as a kind of transcription of African attitudes to the process: "It was funny to M'Cola to see a hyena shot at close range. . . . But the greatest joke of all . . . the pinnacle of hyenic humour, was the hyena, the classic hyena, that hit too far back while running, would circle madly, snapping and tearing at himself until he pulled his own intestines out, and then stood there, jerking them out and eating them with relish. . . . The hyena was a dirty joke" (1935, 37-38).

Aside from its apparent incapacity to play the game of prey right, declining either to fight or die with dignity, the hyena's self-consumption disrupts the basic boundary, parodically rendering hunter and hunted as overlapping definitions. Hemingway's occupation of the African point of view here enables another doubling move, of reiteration and distancing, from M'Cola's capacity to repudiate through laughter a spectacle that Hemingway also finds abhorrent but fascinating enough that he must return to this scene, repeating it with reference to parameters of traditional taboo violation: "the hyena, hermaphroditic self-eating devourer of the dead . . . loping away on the brown plain, looking back, mongrel dog-smart in the face; whack from the little Mannlicher and then the horrid circle starting" (1935, 38). The hyena as object of derision is a handy thing; so long as M'Cola can be seen to laugh at it he need not be seen to be also mocking Hemingway's absurd need to recreate himself constantly as hunter—as the hyena seems to be, producing on its own body the horrid circle that the white hunter must ceaselessly reformulate into a linear hierarchy.

The notion that the hyena is a scavenger, "dangling at the bottom of the food chain," in the words of Whoopi Goldberg's Schenzi in *The Lion King*, is resilient. While all scavengers are devalued for their failure to occupy the normative categories of hunter and prey, hyenas, perhaps even more than jackals and vultures, are the quintessential scavengers of the cultural imaginary. This elevation to typicality is suggested (and presum-

ably also reinforced) by the hyena diorama at the American Museum of Natural History in New York. Every animal or group of animals gets a display in which stuffed specimens stand about in attitudes suggestive of feeding, alert survey of the landscape, sleep, or play. Only the hyenas are featured snacking on a scavenged carcass; clearly the essence of hyena that the exhibit seeks to convey requires that we understand it as defined by this particular act. For the white hunter, invocation of this categorization for the hyena resolves instability into abjection. Roosevelt's accounts of his safaris feature several instances where, observing lions scavenging on hyena hunts, considerable reinterpretation is necessary to preserve the expected hierarchy. The essentially defensive nature of his and Hemingway's reading requires that new data be positioned so as to confirm established relations; it is thus only with the growth of fieldwork as a dominant mode of knowledge production from the middle of the twentieth century that studies have established the extent to which hyenas, particularly spotted hyenas, are predators as well as scavengers and, still more relevant to their negative meanings in human systems of representation, the extent to which lions live by scavenging hyena kills rather than vice versa.

The configuration of the Natural History Museum's exhibit is a material trace of the continuity, biographical and epistemological, between the early twentieth-century collector/hunters who provided the specimens for exhibition and scientific institutions. While the museum is now seen as and promotes itself as a site of current educational and scientific best practice, the unmodified "hyena as scavenger" diorama points up the African Hall exhibits' origins in Rooseveltian concepts of the natural world as a field for the rediscovery and emblemization of both white masculinity and hierarchical models of relation within and between species.¹⁹

It is with fieldwork, funded by research monies, that the hyena as scientific subject begins to emerge—not necessarily as the meaning construct of first resort in a larger, nonscientific public sphere but as at least an alternative to the older, mythified deviant. There have been to date two distinct forms of scientific study of the hyena, that done in the field and that done in the laboratory. While there is considerable overlap in personnel between the two locations, with individuals often coauthoring papers from both at different times, the focus of each is divergent. Fieldwork produces papers on social organization, both within and between hyena clans, and group dynamics. Hyena studies, which had hitherto been

¹⁹ For an extended account of the Natural History Museum's African Hall as a project directed at the salvation of a decadent modernity, see Haraway 1989, 26–68.

based largely in the field, were radically reconfigured in the 1980s by the establishment of a captive colony at the University of California, Berkeley. The transformation of the hyena into a laboratory animal enabled not only new kinds of behavioral study but also new investigations of hyenid endocrinology. The foundation of the BHP has resulted in a shift toward a hormonally based explanatory model of social behavior.²⁰ As we shall see, there are variations within this general trend and a continuing dialogue between field and laboratory, but the prevailing scientific truth story has become a narrative about the search for the originating chemical at the heart of hyena deviance.

There are three species of hyena: brown, striped, and spotted. It is the last of these (*Crocuta crocuta*) that has been overwhelmingly the subject of modern scientific study both in the wild and in captivity—the Berkeley colony is made up of spotted hyenas. The spotted hyena has both behavioral and morphological features that make it an especially attractive and mysterious analysand, features that also suggest continuities with the premodern hyena.²¹ In the wild the spotted hyena lives in clans, hierarchically organized groups in which females in general outrank males. Within a matrilineal system in which daughters tend to inherit their mother's status, hunting and feeding are communal. Female offspring remain with their clan of origin, while young adult males disperse, usually to seek reintegration with another clan after a period of isolation.

The other aberrant quality of the female spotted hyena (not shared by other hyenids) is her physical appearance. To the human eye, the female is indistinguishable from the male in the field because they are not sexually dimorphic, although adult females tend to be somewhat larger than adult males. This similarity is always described in the literature in terms of the females' closeness of approximation to a (genital) male norm: females have a "male-like" "pseudopenis" and "pseudoscrotum." Visually, this is to say, the female spotted hyena's clitoris is hard to distinguish from the male's penis, being about the same size and capable of erection. This physiological peculiarity had the effect of preventing certain types of study; the lack of

²⁰ In the pages that follow I refer to various researchers and their work under the rubric BHP. I intend this as an imprecise term to denote influence: while in many cases the workers in question are faculty, graduate students, or former graduate students at Berkeley, some are only linked by having done collaborative work.

²¹ Most authorities speculate that Aristotle's foundational commentary on hyenas would have been based on observation of striped hyenas: see Glickman 1996, 510–11, for a summary of this argument. However, as Glickman points out, the notion of the hyena as hermaphrodite that Aristotle seeks to discount would probably have had its source in others' experience of the spotted hyena.

visual markers for difference—the fact that males and females could not reliably be told apart in the field until females had given birth or lactated—limited the kinds of observations that could be made about sex-differentiated social organization and intersexual relations.

The first modern studies to focus largely or exclusively on hyenas are markedly hyena positive in Glickman's terms, seeking to redress a perceived injustice done by earlier science and culture.²² Hans Kruuk's landmark 1972 work on the spotted hyena portrays a society of peaceful clan dwellers, where successful mechanisms have evolved to minimize intra-clan conflict. In Kruuk's view the hyenas' is a "fluid" social system, in which hierarchies make fighting over food, or anything else, almost unnecessary. His hyenas are efficient, cooperative predators (a careful case is made for the importance of hyena kills to lions as scavengers), neither aggressive nor cowardly; Kruuk describes both instances where hyenas avoided large carnivores and others where they mobbed them. Overall, the focus of this work is the interaction of hyenas with their environment: while Kruuk notes some of the issues that become central to later studies, such as female social dominance, size, and distinctive anatomy, these are de-emphasized in favor of attention to the hyenas' ecological niche in the bioeconomy of the Kalahari.

Several other field studies of the hyena have taken place, or are continuing, in East Africa since the 1970s. However, some zoologists, desirous of studying the hyena's sex/gender system, lamented the particular obstacles to scientific study presented by the spotted hyena. Fortunately for those committed to the revelation of the secrets of the body, in 1984 a University of California scientist, Laurence Frank, discovered how to differentiate by sex even young hyenas based on a difference in the shape of the erect penis or clitoris—opening the door to a range of studies on newly readable, sexed bodies.²³ This additional scopic capacity, described as "revolution[ary]" (1994, 39) by Frank, occurred contemporaneously with the setting up of the BHP's captive colony at the University of California's Field Station for Behavioral Research. While fieldwork, much of it conducted under the auspices of the Berkeley project, continues, the

²² See, e.g., the chapter on spotted hyenas in van Lawick-Goodall and van Lawick-Goodall 1971, 149–207. This overwhelmingly sympathetic portrayal also illustrates the difficulties consequent on having no means of sexing cubs and young adults: many observations of divergences in behavior and rank among cubs are attributed by van Lawick-Goodall and van Lawick-Goodall to personality but might also be heavily influenced by differences in gender-inflected rank.

²³ For a popular account of this discovery, see Frank 1994. For the hard science version, see Frank, Glickman, and Powch 1990.

captive colony has been the locus for a large body of research and publications, both in scientific journals and the popular press. This research, funded by the National Institute for Mental Health (NIMH), has been organized around the examination of links between hormones and behavior. As Frank's popularizing version has it: "We wanted to see if the female hyena's anatomy and aggression were caused by unusually high levels of androgens" (1994, 39). The captive colony at once signals and enables a move away from the study of social systems toward the study of individual organisms and their endocrinology; the operating theory of causation shifts decisively from group and environmental pressures, constraints, and opportunities to explanations based on internal mechanisms. Ideologically, as with all shifts away from the social toward the molecular, this also appears as a move toward "harder," more quantifiable science.²⁴

The creation of the captive colony greatly expanded hyena studies, enabling types of experiment and observation that are impossible in the wild: the captive animals' environment can be manipulated and controlled, differences in individual behavior can be closely and accurately monitored, and activities hitherto hidden, such as neonatal behavior, are available to view. The founding of the colony also enables some observations about the nature of contemporary hyena studies: the colony comes into existence only when what can be studied under such conditions begins to seem the crucial elements of hyena makeup. In the wild, even the most persistent and skillful observer, armed with Frank's method of sexing young hyenas, will miss much of what goes on, but the inevitable incompleteness of the data is arguably compensated for by its relatively unmediated status: while the observer's interpretive input is unavoidable s/he need have a minimal direct or indirect influence on the subjects' behavior or environment. If what you are studying are the mechanisms of groups, the shifting dynamic structure of a clan whose members operate a complex system of communication, these conditions are essential. But if you are studying an organism that is fundamentally organized by internal secretions, the conditions of the wild are a distraction and an irrelevance.

The nature/nurture binary has been comprehensively complicated and problematized in recent scientific theory and practice. Anne Fausto-Sterling's discussion of what she calls "developmental systems theory," for example, argues persuasively for an interactive model for understanding

²⁴ Donna Haraway (1989, 125) points out that the claim for scientific accounts' superior truth value because of their less ideologically motivated nature is in itself an ideologically based narrative founded on the superiority of rational positivism over other hermeneutical methods.

how hormones and environment, among other variables, influence an individual organism's development (2000, 25–27). The structure of hyena studies at BHP tends, however, to reinforce a hormone/environment dualism. For example, the captive colony was formed through the capture of hyena infants from the Masai Mara National Reserve, so that in addition to the obvious absences of territory or hunting opportunities, the BHP colony initially lacked a clan structure based on adult and matrilineal relations. The removal of such "external" factors as "powerful maternal influence" is seen as enabling a focus on "internal" ones.²⁸ In the reigning hypothesis of hyena behavior, what is left comes to represent the fundamental, core hyena that remains after concealing layers are stripped away.

However, the notion of an autonomously operating endocrinal system is not reliably evidenced by BHP results, for, as the colony's instigators noted, the removal of interactive elements provided by an ongoing intra- and interclan social process turns out to affect those aspects of the hyena imagined as primary. Controlled conditions may only produce "hormonal" effects when the "savanna" is reintroduced: "Artificial test conditions may reveal hormonal effects that are masked by natural contextual variables. But there are also cases in which hormonal effects can only be detected when critical aspects of the field situation are recreated in captivity" (Glickman et al. 1992, 154–55). Yet while BHP work on hyenas often, as here, suggests the necessity for an interconnective hypothesis for interpreting results obtained at the colony, kudos and funding continue to flow toward investigations of hormonal sources of behavior.

The establishment of the BHP and its funding source are structurally significant because they locate the hyena as subject in the ongoing institutionalization of particular theories about how studies of animals bear on our understanding of human behavior and organization. From the beginning, the motivation for study of the captive colony has been the search for hormone-based truths that are generalizable across species boundaries: "Finding an androgenic basis for the 'masculine' traits of the female spotted hyena would encourage a more general exploration of natural androgens in female mammals than has taken place to date" (Glickman et al. 1992, 138).

But behind the hermeneutic power of hormones lie other structuring

²⁸ See, e.g., Woodmansee et al.: "Since the operation of [a] powerful maternal influence would have obscured our attempts to examine the emergence of sex differences per se, hyenas were collected as infants and reared in peer groups, without a maternal presence. Although this is a highly unnatural situation, many fundamental aspects of hyena social behavior emerged in our peer-reared cohorts" (1991, 10).

ideologies that enable the hyena to seem a compelling mechanism for the illumination of human consciousness; we cannot find such biologicistic discourses persuasive unless we have already absorbed a set of other assumptions that underpin them. The possibility of finding truth in hormones alone is plausible only if culture is always already secondary to biology.

Animal studies that seek to extrapolate to humans assert themselves as revelations of the innate, of what is natural and constant between one species and another. Within the generalizing frame provided by this understanding of how the human may be read within and on the animal, the hyena's special function is to reference the abnormal, "One of Nature's Experiments."²⁶ The mode of inquiry that seeks to define norms by reference to that defined as abnormal is a well-established orthodoxy, although the grounds on which the female spotted hyena's abnormality is defined are, as we have seen and as continues to be the case, highly flexible, adaptable to a range of knowledges and cultural requirements. It is notable also that the hyena's abnormality is defined for science as consisting in what Stetkevych (1996), speaking of tribal understandings of the hyena, calls "confusion or inversion of categories": "This species has been chosen for study because it exhibits absence or reversal of many sexual dimorphisms typical for mammals. Female spotted hyenas have no normal external genitalia" (Woodmansee et al. 1991, 10). Where sexual difference is defined as the norm toward which mammals aspire, spotted hyenas transgress both ways at once—by the wrong kind of sameness (ubiquitous penises) and the wrong kind of difference (female dominance). Obviously, this systemic structuring of difference as deviance from a norm will have consequences for any extrapolation from hyena to human.

With the shift from field to laboratory, from social interaction to endocrine function, it is the female spotted hyena's departure from an assumed mammalian norm, not only of hormonal makeup but of behavior hypothesized as stemming from hormonal effects, that comes to define it. The BHP focus on hormones is in step with contemporary developments in studies of other laboratory animals; much work on rats, in particular, established the capacity of androgens to "masculinize" female external genitalia, and a body of work also exists that argues for the organizational role of "sex" hormones in "masculine" and "feminine"

²⁶ "One of Nature's Experiments" is the subtitle of Glickman et al. 1992

behavior.²⁷ The development of the argument for hormones as organizing of both the hyena sex/gender system and individual behavior within that system is not specific to hyena studies. The special nature of the hyena, viewed from a perspective that locates hormones as an individual and social first cause, lies in what the female spotted hyena's genitalia are taken to mean about the hyena's hormonal environment of origin.

The reiterated statement "female spotted hyenas have no normal external genitalia" tells us more than that we have moved from the natural into the socially mediated world of science; it points up the species' unique function as a defect experiment. The classic defect experiment requires that, having decided on a key organizing factor in some form of structure, whether on the level of the molecular or the social, this factor is removed and the results observed, thus establishing both the importance and the effects of the key feature. So, investigating dominance or sex differentiation (popular areas of study and for the use of defect experiments), one might remove the alpha male from a group of primates or perform gonadectomies on some specimens in a group of young mammals. Viewed as "nature's experiment," the spotted hyena is a naturally occurring defect that enables both morphological and behavioral study of the removal of (normal) male dominance and the administration of androgens to females. The female spotted hyena's genitalia signify a more generalized abnormality: there is, that is to say, no normal spotted hyena; each live birth is a little aberration waiting to be studied.

The search for explanations for the female spotted hyena's anomalous sexual morphology begins, in scientific discourse, with the first dissection of hyenas by Watson in the 1870s. The female's genitalia raise two problems for science: causation and function. There are two schools of thought concerning the second question, one that argues for the functionality of an enlarged clitoris in hyena social organization, usually as an essential tool in the particularly complex system of greetings that hyenas appear to use to establish and reiterate clan membership and status. ("Greeting" takes the form of a lower-ranking animal offering its erect genitals to individuals of higher rank for inspection.) The other side argues that, rather than being a development that has been selected for, the female

²⁷ In rats, behavior is defined as gendered by reference to mating maneuvers: mounting is a "masculine" behavior, and lordosis (a receptive posture enabling mounting) is defined as "feminine." For an extended discussion of the significance of the "discovery" of sex hormones, see Fausto-Sterling 2000, chap. 7, "Do Sex Hormones Really Exist? (Gender Becomes Chemical)." For a critique of the rat sexuality research, see Fausto-Sterling 2000, 199–232. For a critique of this work and its extension into sexual orientation, see also, e.g., Birke 1982; and Byne 1995.

hyena's "penis" is a side effect, even if a side effect that has come to have significance socially.²⁸ A theory that begins from the proposition that an erectile clitoris might be actively selected for because of its key role in greeting ceremonies, and hence in peaceably maintaining clan ranking and cohesion, produces a very different concept of the spotted hyena than does one that argues that enlarged genitalia are an unwanted side effect of steroids, and that like the proverbial woman shot-putter with a beard, the hyena has acquired "male" hormones so that she can get large and beat off the competition and has ended up with deformed genitals as a result. (The proponents of the latter hypothesis also point to the relatively high incidence of stillbirths and mortality among first-time mothers as further significant costs of masculinized genitalia: the narrowness of the clitoral opening [there is no other] makes access for mating purposes a matter requiring careful positioning and cooperation between partners, and the clitoris must tear for birth to take place.) If the genitalia are not selected for but are an incidental defect, their source and the mechanism of their production are the more plausibly to be sought in a hormonal first cause. While terminology has changed over the course of hyena studies to date, and the source and identity of the crucial hormone has been open to dispute, it has come to seem clear that the female spotted hyena's genitals are a result of prenatal exposure to androgens. (Without fail, this is described in the literature as the fetus being "bathed" in androgens—a telling example of what was once a lively if exaggerated metaphor hardening into truth.)

The objects of study at BHP are both hormones themselves (where they come from, and when) and the effects of these hormones on particular aspects of the behavior and physiology of the individual subject. Specifically, the project has focused on explaining the presence of high androgen levels in the fetal environment and in neonates and on the sequence of the hormone's presence, absence, and creation—exploring, for example, the passage of different hormones across the placenta, ovarian development in adults, and levels of androgens in pregnant spotted hyenas. The project also seeks to define how hormones affect directly and indirectly

²⁸ The evolutionary question is almost exclusively debated in terms of genitalia rather than, e.g., as a way of discussing the uses of female dominance; the argument for large, erectile clitorises as a side effect that comes, secondarily, to have social uses is set out, e.g., in Gould 1981. Hans Kruuk (1972) proposed the primary usefulness of genitalia for meeting ceremonies, a theory recently taken up and expanded on by Max Planck scientists (see, e.g., East, Hofer, and Wickler 1993) but that may now have lost out to an alternative explanation foregrounding siblicide as an evolutionary mechanism—see below and Goymann, East, and Hofer 2001.

the life cycle and quotidian behavior of the hyena. The key focal points here are female dominance, female aggression, and the construction and social and sexual deployment of the female spotted hyena's "virilized" genitalia. Androgens are the direct cause, in the BHP's working hypothesis, of the females' hypertrophied clitoris and of at least two other traits, one physical—the females' typically larger size relative to males—and one behavioral—their aggressive behavior as both infants and adults. Female dominance, while clearly not a direct effect of hormones, is not consistently differentiated in the literature from aggression: aggression is assumed to be necessary for, and its exercise to lead to, dominance.

The logic of the spotted hyena according to the BHP begins in the fact of female dominance, a deviation from the mammalian norm of male dominance and female subordination.²⁹ For dominance to exist, in this analysis, females must be more aggressive than males, another departure from the mammalian norm, and males less aggressive than females.³⁰ Larger body size is presumed to facilitate aggression and hence successful dominance (although larger females occur in other species, not necessarily in conjunction with female dominance) and also represents another norm deviation.³¹ As a side effect of androgen's primary function, the production of aggression, in this analysis the female spotted hyena's genitalia are merely a sign of the presence of "male" hormones, the "pseudopenis" a flag of the intrinsically masculine nature of aggression. Thus what the virilized female demonstrates is the essential maleness not only of aggression but also of dominance, her "penis" the visible marker of an aberrant positioning in both behavioral and endocrine registers. By the same token, presumably, the penis in its right (male) place naturalizes aggression.

The hormone thesis would in its pure form propose that all behaviors are driven by hormones or conditioned by their absence. For all their

²⁹ The necessity for this deviation, or its usefulness, might lie in the female hyena's responsibility for cub rearing (males take no part in protecting or provisioning clan young), although this hypothesis does not explain why a comparable adaptation has not emerged in lions, say, where cubs are also vulnerable to male predation. See Kruuk 1972, 246, for this hypothesis.

³⁰ It is worth noting that male spotted hyenas are thus produced as deviants too, unnaturally subservient and wimpy in the face of big buff hyena females. For arguments that distinguish between aggression and dominance, see Rowell 1974; Ralls 1976; and Goymann, East, and Hofer 2001.

³¹ For other species in which females are larger than males, see Ralls 1976. For the irrelevance of body size to dominance in other animals, see Rowell 1974; for its irrelevance in hyenas, see Kruuk 1972; Frank, Glickman, and Zabel 1989; and Zabel et al. 1992.

attention to and awareness of environmental factors, the story the BHP publications tell, the impetus driving their collective narrative, is the search for the hormonal grail. In this context, the breakthrough discovery, the one that spurs a series of further experiments and is constantly referenced in subsequent research, is the identification of the endocrinal mechanism whereby testosterone metabolized from androstenedione (a form of androgen) crosses the placenta to become available to female spotted hyena fetuses in utero (Licht et al. 1992). In practice—the practice of experiment design and of writing up research—the focus on the hormonal tends to override environmental possibility, relegating it at best to footnotes, as a typical introduction to an article on the mechanisms of androstenedione production exemplifies.²² Here the assertions of the first sentence, which form the ground for the hormonal truths of the second, are all based on BHP studies (specifically, this extract summarizes the experiment detailed in Licht et al. [1992]) that, read in full, negotiate between hormonal and environmental causations for these phenomena while always privileging the former. But there is no place for “externals” when the research is put to use: “Female spotted hyenas are also heavier than male hyenas, engage in as much rough-and-tumble play, are more aggressive than males, and are dominant in competitive situations. This array of morphological and behavioral characteristics suggests that the females are exposed to high concentrations of androgens during fetal life” (Yalcinkaya et al. 1993, 1929).

The reproduction of this hypothesis-as-truth is a baseline from which other similarly theorized studies flow: an endless series of research grants are funded on the basis of the primacy of hormones to both morphology and behavior.²³ At the same time, the same researchers know that hor-

²² Given the shift toward a hormone-based truth model in current animal research, BHP work and hyena study in general are exemplary rather than exceptional in this regard, except insofar as the perception that female hyena fetuses encounter “excess” hormones tends to heighten further the emphasis on hormonal causation to the detriment of other factors.

²³ A recent research proposal for the spotted hyena from Stephen Glickman, e.g., reads, “Four lines of research are proposed for the next grant period. 1. Experiments in which anti-androgens are supplied to pregnant females, designed to determine the maximal possible effect of such treatment on urogenital development, postnatal steroid secretion and social behavior. 2. Investigations of normal fetal urogenital development and steroid influences during the earliest stages of sexual differentiation. 3. Studies of structures/systems in the hyena central nervous system, which are sexually dimorphic in other species, in order to determine the extent to which such differences are attenuated, or reversed in hyenas due to the high concentrations of pre- and postnatal androgen circulating in both sexes. 4. Studies of exogenous steroids supplied to ovariectomized female hyenas in order to determine whether the exceptional aggressiveness of female hyenas toward males, and the high levels

mones do not really do what the research proposals claim, but this latter knowledge is held apart, having little impact on the institutional perpetuation of scientific orthodoxy: "Actually, we haven't been able to do nearly as much with studying hormones and aggression as we had hoped. . . . Aggression always takes place in a social context, and when you change hormone levels in a social animal, the effect may not be immediate and it may not be obvious, so it's very difficult to measure. The social context may be inhibiting it or correcting for it somehow" (Frank, quoted in Slack 1999, 4).

The capacity to hold to a belief in hormones as organizationally supreme while maintaining awareness of other, competing, theories that do not fundamentally impinge on one's worldview or professional practice is not a sign of a conspiracy in action. It is, rather, a testament to hormone theory's ideological power at this moment in science. A thorough reading of the BHP literature provides plenty of evidence that behavior, even under controlled conditions and certainly in the field, is not solely fueled by androgen exposure and production. A theory that produces dominance as a result of androgen-induced body size and aggression cannot account for a hierarchy that allows smaller animals to rank higher than large ones or that provides for cubs from high-ranking matriline to outrank adult females.³⁴ It also seems inadequate to deal with the difference between the ranking systems that operate among young cubs living at the hyenas' communal den, in which lineage and sex do not seem to be decisive factors and where males often dominate females (a pattern intensified in the early studies of the captive colony, made up initially only of adolescent and younger animals), and the hierarchy into which cubs must insert themselves when integrating into adult social systems, a process that may entail losses or gains in status not correlated with the behaviors that cemented a particular place in the juvenile hierarchy (Frank, Glickman, and Zabel 1989; Smale 1995).³⁵ Several studies acknowledge that social structures

of affiliative behavior observed in dominant females is due to ovarian secretion of estradiol and/or androstenedione." This quotation was taken from the animal rights Web site <http://www.vivisectioninfo.org/oca.htm>. For the results of some of this proposed research, which suggests the limitations of the androgen-driven explanatory model, see below.

³⁴ For the data on the irrelevance of body size to position in male or female hierarchies, see, e.g., Frank, Glickman, and Zabel 1989; for cubs' inheritance of maternal rank, see Holekamp and Smale 1991.

³⁵ Various studies have shown that in juvenile hierarchies, where maternal rank is not a strong factor and adult females are frequently not present, males often dominate. Males lose this position as a result of dispersal: when they enter a new clan it is at the bottom of the hierarchy. That dominance is a function of social position, whatever hormones may also be doing, seems fairly clear from this data.

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influence individual hyena behavior and that rankings are a product of a combination of factors, including matrilineal position, changes in negotiated social relations between individuals, and the effect of coalitions, which occur, for example, when a group of midranking females combines to dislodge a higher-ranking one.⁸⁶ In other words, the possibility of cooperation reemerges, where androgen-based analysis can infer action and reaction based only on competition. And the addition of inherited status and coalition building as factors suggests that hyena organization has something resembling a history and a politics.

All these possible complications are readily available to the attentive reader of the published material. The intermeshing network of BHP journal articles gives a sense of complex interactions, for while the observing scientists set out to document the production of female dominance and aggression by androgens, they often report data that does not support, or even might be said to undermine, these hypotheses.

In addition, while the BHP functions as the hegemonic institution in contemporary hyena discourse, other studies provide a counternarrative to the conclusions drawn from their data. A series of reports from fieldwork done in the Serengeti revisits a raft of reigning orthodoxies, distinguishing female dominance from the qualities that typically in BHP work are elided with it. They note, for example, that dominance is unrelated to size either in other mammals or in spotted hyenas, that dominance is not a function of aggression, that androgens as an organizing mechanism for aggression is problematic in spotted hyenas, since they are not present in notably elevated levels in adult females, and that "masculinized" female genitalia can be disconnected from dominance, since, while these traits occur together in the spotted hyena, each occurs separately in other mammals.⁸⁷ Most fundamentally, perhaps, this school of researchers has proposed an alternative to the side-effect theory of American orthodoxy that fixes female spotted hyenas' genital difference as an unfortunate evolutionary

⁸⁶ See Smale 1995; see also Glickman et al. 1992, 154: "Our studies have emphasized the extent to which expression of sexually differentiated traits is dependent upon the environment in which they are examined. . . . This is . . . the case for the emergence of female dominance in hyenas, where dispersal of individual males places them at a serious disadvantage when confronting large, aggressive females, capable of working in coalitions, on the home territories of the females. In addition, any effects of androgens on sexual dimorphism prior to dispersal are overwhelmed by the power of matrilineal associations and acquisition of maternal rank."

⁸⁷ See East, Hofer, and Wickler 1993, 366–67, for a concise statement of the contrarian position on these issues. For data on androgen levels in adult females and the proposal that high androgen levels are only significant neonatally, see Goymann, East, and Hofer 2001.

accident.³⁸ Marion East, Heribert Hofer, and Wolfgang Wickler argue that the erectile clitoris was selected for and that it functions to restrain male control of reproduction: "forced copulations are impossible. Females thus gain control over the mechanisms of copulation and male mating success becomes dependent on the relationships they develop with females" (1993, 368).

The implications of such a hypothesis are rich. But while the possibility of the female spotted hyena's rehabilitation from her current status as an experiment that failed is refreshing, and the temptation that this theory offers to imagine an alternative "natural" model for human heterosexual relations may be hard to resist, the impact of this revision on either the scientific or the popular hyena has been minimal.³⁹ A consistent element of the BHP's mode of production has been the regularity and frequency with which research written up in scientific journals is reformatted for publication in popular science magazines, both by journalists and under the byline of the researchers themselves. The success with which BHP research findings have been reproduced in the mainstream press has ensured that a particular version of the hyena story circulates. Further, in the popular accounts the nuances in interaction carefully acknowledged by researchers between direct and indirect hormonal effects, and the importance of context and environment, are suppressed.⁴⁰ While this process is not unique to hyena studies, the results in this case are particularly sensationalist: the news is hormonal, of "male-like moms," "freakish sexual chemistry," "bizarre sexual habits," and "high levels of testosterone . . . toxic to the eggs . . . developing inside hyena fetuses."⁴¹

Further, an already simplified hormonal explanatory model also forms the basis for large extrapolations; the popular accounts home in unrelentingly on the angle assumed to be the most interesting to readers: the comparison between hyena and human. Much, in particular, is made of comparisons with a 1991 study that links pseudohermaphroditism in a human female infant to metabolization of high levels of androstenedione, especially of the observation that her fetal environment resembled that

³⁸ It is a school in the sense that papers challenging the BHP position emanate from the Max Planck Institute, Germany.

³⁹ Frank dismisses this idea as "the chastity belt theory"; see Slack 1999. It would seem to have been abandoned by Max Planck researchers also, in favor of selection for neonatal aggression; see below and Goymann, East, and Hofer 2001.

⁴⁰ The *Boston Globe* account of juvenile ranking systems and the reassignments that occur as subadults integrate into the clan, based on Smale and Holekamp's research and on Frank's siblicide data, is an exception to this; see Croke 1992.

⁴¹ *New Scientist* 1985, 29; Brown 1993, A2; Stevens 1993, 6D; *Berkeleyan* 1995, 1.

encountered by the spotted hyena fetus: "The woman's placenta was acting just like a hyena's" (Stevens 1993, 6D).⁴² In this example, as is usually the case, the telling quote comes from a BHP scientist. Reports also pick up on a comparison between the ovaries of the female spotted hyena and those of women with polycystic ovaries: "the hyena's stunted ovaries provide clues to human polycystic ovarian disease. Women with the disease have difficulty becoming pregnant. . . . Their ovaries, in fact, look just like hyena ovaries" (Stevens 1993, 6D).⁴³ Despite the fact that the hyena's "stunted" ovaries do not appear to lead to infertility, we are left with little doubt that there is something fundamentally wrong with spotted hyenas and that the quintessentially aberrant hyena is highly germane to understanding the operation of human females. The link made between human and hyena here neatly equates difference with disease and infertility with gender aberrance.

A typical transference of defect from hyena to human occurs in the account of early findings from the BHP in *Psychology Today*, which features a sidebar that in setting out the standard literature on the production of human intersexes also makes explicit an assumed connection between fetal hormone exposure and sexual orientation (never made in BHP scholarly articles or in the researchers' popularizing accounts of their findings); it is titled "When Hormones Go Wrong" (Hopson 1987).⁴⁴ The title of the main body of this article is also worthy of note: "Boys Will Be Boys, Girls Will Be . . . Female Spotted Hyenas look and act like males. They may help answer questions about gender and aggression in humans" (1987, 60). Female spotted hyenas who act the way they do can only be assimilated into a model of masculine behavior (boys will be boys), for this is how norms and deviations from them function: the category "female behavior" cannot be enlarged to include the matrilineal spotted hyena because she has already been designated as that which the norm is defined against, and hence she can affect it only to reinforce existing limits. The ellipsis allows, suggestively, for the same claim to be made implicitly about "girls": boys are always already ontologically defined, safe (because of hormones?). Girls who "look" or "act" like boys, however, will become

⁴² The quotation is attributed to Stephen Glickman. The report on female pseudohermaphroditism referred to is Shozu et al. 1991.

⁴³ The original study referred to is Yalcinkaya et al. 1991. The comment quoted here is reported as indirect speech and attributed to the second author of the study, Pentti K. Siltari.

⁴⁴ The argument for a link between fetal exposure to hormones and sexual orientation in humans is made, e.g., in Friedman and Downey 1993; for critiques of this hypothesis, see Bailey 1995; and Banks and Gartrell 1995. For a discussion of the problems of extrapolations to humans of work on rat "sexual orientation," see Stein 1999, 171-74.

. . . boys? Hyenas? Or just something floating in unsignifiable space? The potential for "going wrong" is all, in any event, on the female side of this parallel construction. While the popularizing translations, and the tendency of these translations to exaggerate the original studies' weighting toward endocrinal explanations, are obviously not under the control of the researchers, it would appear from the frequency with which they are available to be quoted making comparisons that the direct extrapolations made between human and hyena females are considered appropriate uses of BHP findings.

On murderous infants and the female penis

Two subjects of current hyena research are particularly useful for thinking about how the hyena now functions as representation and for understanding how that representation is underpinned by, and yet floats free of, contemporary scientific explanation. The authority lent by science and the "hyena's" capacity to work simultaneously on other levels of meaning and connotation than the straightforwardly scientific make for a culturally powerful combination. These research topics—hyena cubs' neonatal aggression and the morphology of the female "penis"—have both been the locus of recent scientific discoveries, though these new developments have not necessarily defined their life in discourse.

Spotted hyenas give birth to singletons or twins, more rarely to triplets. In the wild, the newborn cubs crawl deep into the tunnels of the dens hyenas use as nurseries, emerging only to feed and otherwise remaining underground, safe from predators. As hyenas occupy dens created by other, smaller creatures rather than dig their own, all parts of the dens but the entrances are inaccessible to adult hyenas. Field observation has shown that surviving cubs are often singletons or mixed-sex twins. Only at the captive colony did it emerge that twins and triplets often fight soon after birth and that since hyenas are born with teeth, weaker siblings, prevented from reaching the den entrance to nurse, often die of their wounds. Mortality seems to be especially heavy in same-sex twin pairs.

The scandal of infant hyenas' tendency toward violence first broke in *Science* in March 1991 (Frank, Glickman, and Licht 1991) and was subsequently the subject of articles in *Natural History* and *New Scientist* in 1993 and 1994. In both instances the popular versions were written by those directly involved in the research: Frank authored the *New Scientist* piece and collaborated with Glickman on the *Natural History* one. Since the first report this finding has been given high prominence, infant violence almost threatening to push genital weirdness and adult female ag-

gression off the hyena front page. The tone of the Frank and Glickman pieces is at once sensationalist—they are titled “Born to Kill” and “When Hyenas Kill Their Own,” the second “Kill” set off in enormous point size—and pained. In both accounts, the authors describe distressed researchers (and their wives) nursing fragile, injured cubs back to health, mystified by the source of the cubs’ sickness, and the humans’ inability to anticipate the horrors that improved surveillance techniques at the captive colony finally reveal. Frank writes as a disappointed patriarch whose fatherly hopes have been rudely dashed by a disobedient people: the hyenas are described as having “Cain-like tendencies” (Frank and Glickman 1993, 46; Frank 1994, 38). The behavior seems to incite unself-consciously judgmental pronouncements: “the species’ remarkably precocial infants[’] . . . first actions are not the least bit infantile” (Frank and Glickman 1993, 47).

“Born to Kill” is published as an insert within a longer article by other BHP researchers, “Growing Up in the Clan,” which provides a lushly illustrated account of clan life in the wild, focusing on how juvenile individuals learn to operate within, and find their ranking in, hyena social systems (Smale and Holekamp 1993). The insert is set off from the rest of the text, against a dark background color, and is positioned in the middle of the article so that one has read several pages about the happy matriarchal clan before coming across the shocking secret at its heart. While “Born to Kill” resonates invitingly (with, e.g., the hyena’s traditional other in the good/evil binary, the “Born Free” lion), the inner text’s placement reiterates the powerful primary role of hormones in determining behavior—and our evaluation of that behavior. As researchers point out, siblicide is close to an event that has no cultural or environmental aspect: straight out of the womb, separated from their mothers, if cubs fight it would seem as if they must be “preprogrammed,” presumably hormonally, to do so.

In the *New Scientist* piece and subsequently, Frank offers two theories about the occurrence of this phenomenon—explanations seeming necessary not just because such aberrance needs explaining but also because killing siblings does not on the surface seem like a good strategy for reproductive success. The first of these is that precocious infant violence is another side effect of the fetal androgen “bath”; in the desperate quest for dominance (an effect that androgen and only androgen, in this argument, can provide her), the female spotted hyena sacrifices yet more offspring to add to those killed by the difficult birth through the “penis.” Frank offers the second-level explanation that adult female hyenas may

be countenancing or even promoting siblicide in order to adjust cub sex ratios.⁴⁵

The function of the hyena as explanatory mechanism for behaviors far distant from hyena cubs mauling one another in a dark burrow is rendered authoritative by science, by the power, for example, of the BHP's discovery of siblicide to stand as revelation of a hidden but ultimate cross-species truth; but this function is to a significant extent not constrained by the scientific data itself. For example, Frank's reiterated suggestion that adult female spotted hyenas exploit neonatal aggression for their own purposes is an extrapolation from studies that show variations in sex ratios in cubs, seemingly in response to changes in environmental conditions (Holekamp and Smale 1995). This extrapolation is structured by Frank's attempt to make sense of what seems to him another hideous aberration in hyena organization and represents only one possible response to the data.⁴⁶ There is so far little clear evidence that mothers have any control over who kills or injures whom in the natal den in the first few hours after birth, and much that suggests that, given maternal supervision, infants do not die.⁴⁷

⁴⁵ The popular accounts typically either summarize Frank's own popular accounts or carry speculations clearly solicited by the writer; e.g., "Frank thinks that even though the mother cannot enter the den where the siblings are battling, she may be influencing the outcome. He doesn't know how" (Croke 1992, 32). This remains Frank's preferred hypothesis: see Millius 1999; and Slack 1999.

⁴⁶ The alternative hypothesis that sex ratios are determined prenatally is raised, e.g., in Smale, Holekamp, and White 1999. For an unusual example of a popular article that represents hyena siblicide as a matter for divergent opinions among researchers, see Millius 1999.

⁴⁷ Frank gives an example of maternal intervention that he observed, which he describes as evidence of a female adjusting the sex ratio of her litter. From the description he gives (although the original data may be more conclusive), all one can conclude is that the mother attempted to prevent a cub's death: "one of these [triplets], the dominant sibling, was unscathed, while the second bore the scarred shoulders typical of a subordinate cub. The smallest was terribly injured, the skin virtually stripped from its back by its siblings. We saw [the mother] alternating between two den holes, in one of which she seemed to have stowed the runt. Once we saw all three cubs together. [She] was persistently snapping at the dominant cub, trying to prevent it from attacking the injured runt as it tried to nurse" (1994, 41). Perhaps female hyenas do manipulate sex ratios by allowing or preventing siblicide; there are many examples of other mammals where the young are not safe from the predations of their parents. But I am suspicious about a theory that would undercut the female spotted hyena's last claim to "femininity," that she is a "good" mother. This attribute is in itself a puzzle for Frank, who claims that this capacity must mean that the fetal exposure to androgens does not "sex" that part of the brain allocated to maternal behavior: "Interestingly, female hyenas behave normally in reproduction and are excellent mothers, showing that the essential 'female' parts of the brain are protected from the androgens that masculinise their aggressive behaviour" (40).

Similarly, the current orthodox explanation that infant aggression is an unfortunate side effect of the aggression- and dominance-promoting androgen "bath" automatically—if understandably—pathologizes siblicide.⁴⁸ Alternative possibilities—that, for example, siblicide might be advantageous to reproductive success, allowing a single surviving cub better hopes of reaching maturity or competing successfully as an adult—make little impact.⁴⁹ The mechanism at work here is illuminated by comparison with the tone of treatments of other species where neonatal siblicide occurs and where such behavior is always explained as an evolutionary mechanism designed to maximize reproductive success.⁵⁰ Because the female spotted hyena is now produced as a hormonally constructed defect experiment, however, the default explanation for her behavior is that it, too, is a manifestation of chemically fueled deviance.

The ease with which the hyena can figure simultaneously as society's twin evils, Bad Seed and Bad Mother, should give us pause. While scientific discovery (of hormonal excess and its consequences) appears to structure this perception, the dense evocation of taboo strongly suggests that the modern female hyena still compactly references very ancient and profoundly defended cultural boundaries. A peculiar rhetorical aspect of modern hyena research, as that research is written up for scientific journals, is the frequency with which the history of hyena representation is invoked; quite a number of hyena studies papers begin with a reference to Aristotle or Hemingway. While the overt gesture here is one of differentiation and repudiation, it also serves to call up the hyena as hermaphrodite demon. In place of the old scandals of grave robbing and liminal shifting across human/animal boundaries, we have the fresh horrors of violent babies and females with penises, new stories carefully contextualized to shadow forth, even as they banish, the old, as if what is being revealed here is a nugget of transhistorical truth.

The pattern of epistemological breaks and continuities in hyena rep-

⁴⁸ See, e.g., Mealy 2002: "The super-androgenised spotted hyena newborns start out with the tools, skills, and personalities of teenage psychopaths" (1).

⁴⁹ Hofer and East 1992 supports Frank's theory of maternal intervention. But the Max Planck school typically combines a version of this theory with the proposal that high androgen levels are an evolutionary mechanism selected for to produce aggressive neonates; see East, Hofer, and Wickler 1993, 367; and Goymann, East, and Hofer 2001. This combination renders both the presence of androgens in neonates and siblicide useful adaptations rather than errant side effects. (The difference that being a singleton makes to adult size and success is noted by BHP authors but not found conclusive.)

⁵⁰ The majority of recent work has been on boobies; see, e.g., Tarlow, Wikelski, and Anderson 2001.

resentation, as well as the hyena's key cultural work, is most clearly illuminated by a consideration of the female hyena's phallus. The difficulty of distinguishing male from female spotted hyenas because of a lack of sexual dimorphism emerged as crucial as the hyena became a subject in scientific discourse. This confusing sameness is described in the scientific literature by reference to the female "pseudopenis" and in more general terms to "masculinized" genitalia. As we have seen, the discovery of androstenedione metabolization in utero (the androgen "bath") produces a convincing narrative of hormonal cause and "masculine" effect: the female spotted hyena's genitals are "virilized" by "excess" androgens. And, finally, the phallic female hyena proceeds to dominate and be aggressive, just as we would expect. In recounting this story I have deliberately blurred the distinction between the phallus and the penis. What the hyena has, if she has anything, is the former: she is perceived as powerful, and her phallic appendage operates in both scientific and popular sign systems as that power's sign.

So ubiquitous is both the phallic attribution and the assumed direct relation between hyena phallus and hyena penis that it is hard to remember that the female hyena's "penis" in fact is not one in any ordinary understanding of the term. While it contains a urethra, it serves as a birth canal and as a means of entry for copulation, neither of which are penile functions. It could be called an "external vagina" or—the usual alternative, given its erectile capacity—a "clitoris."⁸¹ But it is a "penis" because it looks like a penis; and also because it is, for purposes of scientific explanation, a phallus. But is it a phallus in the hyena sign system, or only in that of humans? And what should we make of the fact that humans attribute phallic power to the female hyena's clitoris?

East, Hofer, and Wickler (1993) suggest that the female spotted hyena's

⁸¹ In response to the earlier draft of this article, one of the *Signs* reviewers argued that the female spotted hyena does have a penis, because only a penis completely encloses the urethra, while females with a clitoris have a separate urethral opening. This fascinating reading productively problematizes, as s/he points out, the idea that only males have penises (and from a rather different source than human transgender theory and practice). But I am not convinced either that a penis has, by definition, to have a urethra within (such a formulation would leave infants born with severe or moderate hypospadias without a "penis," when surely we would want to argue that for such people a penis is still a penis, anatomically and philosophically), or that a clitoris with a urethra is not a clitoris (even though such an anatomical formation might cause an XX human infant to be classified as a hermaphrodite). In my view, organs that are the site of penetration for the purposes of procreation, and through which birth takes place, are probably not penises. The larger question of females with penises, and their potential part in dismantling a binary gender system, deserves a more extended discussion than is possible here.

clitoris is not a phallus in hyena signification: the argument of "The Erect Penis Is a Flag of Submission" neatly reverses our understanding of penile meaning, proposing that subordinate animals use erection to demonstrate not sexual or social potency but their vulnerability to attack. (In this interpretation of meeting ceremonies, the subordinate is required to place his/her engorged genitalia within millimeters of a set of powerful jaws.) The flaccid member of the superior animal represents her authority, for the gesture is not reciprocated.⁵³ This highly innovative reading, while it has been greeted with resounding silence in hyena studies, is nonetheless invaluable for what it suggests about the process whereby animal structures are mapped onto human ones, and vice versa. Our own emphatic association of the erect penis with phallic power makes it very difficult to imagine a world in which erection might denote the performance of helplessness and where potency might be expressed (barely a linguistically available concept) by nontumescence. One might, next, have to imagine a social structure whose signifiers of authority are not always already masculine.

Looking at recent BHP work, there are other hints that the hyena might not be simply a phallic pretender. The "penis" comes about, according to the BHP, as a result of androgen-induced masculinization in utero. However, recent experiments seem to show that this is not what happens: if you take away the androgens, the fetus will still develop her "male-like" appendage (Drea et al. 1998).⁵⁴ Another study suggests that the female spotted hyena's characteristic clitoris appears to form prior to androstenedione metabolism (Licht et al. 1998).⁵⁴ Whatever the androgens are "for," they do not seem to be producing the "penis" by mistake as an accidental by-product. If the enlarged clitoris is not produced by the "male" hormone androgen, the female spotted hyena's position as "masculinized" is, at least, questionable. Again, there is little sign of such questioning in the literature; the hypertrophied clitoris's possible independence of androgen has not yet prompted the articulation of an

⁵³ I am aware that one could argue that a system that relies on erection or its absence to signal and maintain hierarchy, even where inverted, is still a phallic system; but this might not be the case if what is being offered is essentially a conveniently available and biteable body part, a kind of synecdoche for the throat.

⁵⁴ The experiment does alter the appearance of the hyena's clitoris somewhat but does not result in the hyena's acquiring "normal" "feminized" genitalia.

⁵⁴ A further experiment in this series links the development of the female spotted hyena's characteristic clitoris to estrogen; see Glickman et al. 1998.

alternative social model.⁶⁶ This might suggest that, if the news about the clitoris has not dephallicized the hyena, it is probably because the hyena with the phallus is a much more useful sign.

The phallic hyena is a beautiful, economical embodiment of female dominance because she so exactly re-presents dominance as an inherently masculine quality. The quintessential deviance of female appropriation of power is eloquently displayed both by the female hyena's deformed phallicized body and by the manifestly abhorrent behaviors of which, whether as cub or as adult, she is capable. Both the reiteration of the phallus and the historical thick description of the hyena's place as anathema in the human cultural imaginary serve to lock the hyena in place. But this solidification works against human female possibility, too: one of the insidious aspects of contemporary popular deployments of hyena studies material is the way in which, while apparently discussing an atypical mammal, we find that we are by unexamined analogy referencing a universal woman's body. Only a determined defamiliarization of that rhetorical move (and it is far from a new one), coupled with a disconnection of female power from the phallus, could begin to suggest a different lexicon. But the hyena could be a means to expansion of scientific thinking, beyond binary conceptions of gender or unitary models of understanding behavior. While this does not seem to be the direction in which contemporary hyena studies are tending, the hormone story would appear to be under some pressure, from within and without the BHP. The historical hyena's transgressive movement between categories translates into a disinclination on the part of the laboratory subject, apparently, to produce the results that are expected of her and her endocrinology. Perhaps, if it is to be found nowhere else, one can take comfort in the transhistorical occurrence of resistance: the hyena is still laughing, surviving a move from the borders into the heart of the culture of surveillance.

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⁶⁶ The resilience of the androgen causation model is evident in recent popular accounts that, although usually heavily dependent on BHP research and researchers, either do not mention the new findings or give them little credence. See, e.g., Slack 1999; and Somon 1999.

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Science, Power, Gender: How DNA Became the Book of Life

Natural science, which is what we usually mean by *science*, involves interacting with nature in ways that produce certain kinds of interpretations of how nature works. There are different styles of doing science, depending on what aspects of nature scientists are exploring, but all of them are constrained by rules of what constitutes evidence and what conclusions are considered permissible. The different ways in which scientists pursue their work, however, do not easily fit into male/female categories.

The fact that, in the United States, many more physicists are men than women has to do with the social and economic structure of domestic life, with processes of education and professionalization, and with the social history of the disciplines, not with the intrinsic nature of physicists' knowledge base or the nature of women and men. We live in a gendered society, and it should not surprise us if women and men tend to develop different tastes in the kinds of knowledge they seek and the ways they seek it, but this fact cannot be used to predict the practices of individual scientists. Besides, science imposes a hegemony within which all its practitioners must operate if they want what they do to be acknowledged as science.

In this essay, I want to describe the contributions two outstanding women scientists have made, in the course of the twentieth century, to our understanding of how genes function and to raise the question of what ways gender can be said to have entered their scientific accomplishments and careers. But, first, I need to review briefly how, during this period, genes and DNA have come to be the iconic objects they currently are.

I
Genetics, of course, starts with the Czech monk Gregor Mendel. Using pea plants as his experimental objects, Mendel examined the transmission of flower color and of the shape and texture of the seeds to successive

This essay is based on a talk, given in February 2001, as part of a series of lectures organized by the Radcliffe Institute for Advanced Study on "Feminism and Science in Civil Society."

generations. He deliberately chose discrete traits, such as red or white petals, or smooth or wrinkled seeds, rather than traits that vary continuously, such as weight or size. He also only kept track of the mathematical regularity with which these traits are transmitted and did not speculate about what processes inside the plants are involved in transmitting the traits. He simply assumed that "factors" inside the plant mediate their transmission.

The publication of Mendel's paper, in 1865, provoked little notice. But by 1900, when the paper was "rediscovered," it aroused immediate interest. The reason is that, in the intervening years, scientists had learned a great deal about the internal structure of cells and about what happens when a cell divides and gives rise to two daughter cells. Stainable bodies, called chromosomes, had been observed in the cell's nucleus, and scientists had noted that different cells of the same organism all contain the same number of chromosomes. Scientists also noted that, when cells divide, their nuclear chromosomes split in two, which is how each daughter cell ends up with the same number of chromosomes as were present in the parent cell. On the basis of such observations, by 1900 biologists accepted that chromosomes have something to do with the way traits are transmitted from parents to offspring, and Mendel's hypothetical "factors" came to be conceptually associated with the chromosomes.

The Danish botanist Wilhelm Johannsen, in 1905, invented the word *genetics* to signify biological inheritance, and, in 1909, he coined the word *gene* to lend more concrete reality to Mendel's "factors." At a time when invisible atoms and quanta were being accepted into the world of chemistry and physics, biologists had little problem accepting that heredity is mediated by invisible material particles, carried on the chromosomes. And soon, as biological chemists came to identify all sorts of molecules that function in cells, one of the questions they tried to answer was what kinds of molecules the chromosomes and genes are made of and how they function.

Once chemical analyses had shown that chromosomes contain two types of very large molecules, proteins and DNA, some scientists suggested that DNA provides the chromosomes with a structural framework to which proteins attach themselves to form the genes. What gave this model plausibility was that DNA, though very large, is a relatively simple molecule, containing large numbers of just one kind of sugar and phosphate and four different kinds of the "bases," whose designations—A, G, T, and C—have become part of our ordinary vocabulary (as has the acronym DNA). It seemed hard to imagine how combinations of only these six subunits could specify all the different characteristics organisms inherit

from their parents. Protein molecules, by contrast, are composed of some twenty different subunits and come in many different shapes and sizes. It therefore was logical to assume that genes were made of proteins.

By the early 1950s, however, experiments with bacteria and viruses showed quite clearly that heritable characteristics are transmitted by DNA, not by proteins. Thus DNA was generally accepted as the substance that mediates inheritance—in a word, the gene.

II

This brings us to April 1953, when three papers appeared side by side in one issue of the British science weekly *Nature*. The first, from Cambridge University, was coauthored by James D. Watson and Francis Crick; the other two, from King's College, London, were authored by, respectively, Rosalind Franklin and Maurice Wilkins with their coworkers. The Watson and Crick paper announced the now-familiar double-helical structure of DNA. The other two offered evidence in support of this structure. James Watson has described how he and Crick arrived at the DNA structure in his best-selling memoir, *The Double Helix*, published fifteen years later, in 1968.

What immediately got scientists excited about the Watson-Crick model was that it can be made to explain how DNA—"the gene"—gets copied when cells replicate. The point is this: Let us picture the double helix as two railings of a spiral staircase, each of which is composed of a long, invariant sequence of sugar-phosphate-sugar-phosphate-sugar-phosphate units. The two railings are connected by a regularly spaced series of rungs, which make them run parallel to each other. Each rung is composed of a pair of bases, and the geometry of the double helix is such that, for two bases to form a rung, an A on one railing must meet a T on the other and a G on one railing must meet a C on the other.

This geometrical requirement means that, when cells divide and their chromosomes and genes get copied, the two strands of the double helix need merely unravel bit by bit. The sequence of bases on one strand then specifies the base sequence for the synthesis of its partner. Thus, DNA ("the gene") gets copied by virtue of the requirement that an A on one strand of the double helix meet a T on the other and a G on one strand meet a C on the other, an incredibly simple and exciting outcome.

Yet this very simplicity conceals a conceptual trap, because it led scientists to describe DNA as a "self-replicating" molecule. And this has endowed the gene with the supposed power of not just participating in the metabolic and synthetic activities of cells and organisms but of mas-

terminating and directing them. But, of course, DNA does nothing of the sort. Without the metabolic activities of cells, DNA is neither copied nor does it participate in specifying traits. Indeed, left to itself, DNA is one of the most inert and stable molecules in biology, which is why it can be isolated, still intact, from ancient fossils.

Only by ignoring the participation of the rest of the cell and organism have molecular geneticists enshrined the magic of DNA—the autonomous, all-powerful gene that does not just specify traits but produces and controls them. The fact that biologists, who are not usually known for their religious commitments, have selected “the Holy Grail” and “the book of life” as their metaphors for DNA—not to speak of President Clinton’s referring to DNA as “the language in which God created life”—underlines the ideological content of molecular genetics.¹

III

What relevance does all this have to gender? To answer this question, I want to look at the contributions two outstanding women scientists have made to our understanding of genetics and DNA. I refer to Barbara McClintock and Rosalind Franklin. I have written about Franklin’s contributions before (Hubbard 1990, chap. 5), but as DNA has come to occupy not only a central role in biology but a larger-than-life role in the culture, certain elements of both her story and McClintock’s story have taken on new significance.

Born in 1902 and dying in 1992, Barbara McClintock’s life spanned the twentieth century. She earned a Ph.D. in botany from Cornell in the early 1920s and stayed on at Cornell’s College of Agriculture on fellowships for several years, working on the structure of the cells and chromosomes of corn (maize) and on its genetics. At Cornell, McClintock had access to a good-sized plot in which to breed corn. She needed that because she felt she had to get to know the individual, living plants if she was to make sense of what she observed when she later studied the detailed structure of their chromosomes under the microscope.² When it became clear that she was not going to be offered a position on the Cornell faculty,

¹ “Reading the Book of Life: White House Remarks on Decoding of the Genome,” *New York Times* (June 27, 2000), 8.

² My information about Barbara McClintock comes largely from Evelyn Fox Keller’s McClintock biography (Keller 1983), from McClintock’s Nobel Lecture, from conversations with some of her friends and colleagues, and from a conversation I had with her in the early 1980s.

McClintock began to look around and eventually ended up accepting an assistant professorship at the University of Missouri. There she spent a few scientifically productive, but otherwise not very satisfying, years. The facilities were not all that good, so she needed to maintain her plantings at Cornell and shuttle back and forth. She also did not interact too well with some of her colleagues, nor did she particularly enjoy teaching. When she was passed over for promotion, she felt it was time to move on. With strong support from older, established (male) colleagues, McClintock was invited to spend a year at the laboratory of the Carnegie Institution at Cold Spring Harbor, Long Island, and it became her permanent home. McClintock's lack of academic success did not stand in the way of her recognition within the profession. She was elected vice president of the Genetics Society of America in 1939 and its president in 1945. More important, she was elected to the National Academy of Sciences in 1944—only the third woman member since its founding by President Lincoln. And she won a Nobel Prize in 1983—only the second woman scientist to win an unshared Nobel, the other being Marie Curie.

From the start, McClintock made pathbreaking contributions. But since she was committed to looking at genes in the context of the whole organism, which was not the usual perspective in her field, many of her fellow geneticists simply did not understand her experiments or the way she interpreted them. When she concluded that genes can change their positions on the chromosomes, along with their functions, in response to changes within the plant and around it, this was so contrary to what geneticists believed possible at midcentury that many of them simply wrote her off. Not until the 1970s and 1980s, when comparable observations were made with bacteria, was what McClintock had been saying accepted into the canon of the field.

So what does any of this have to do with gender? Certainly, McClintock's failure to be promoted within academia had a lot to do with it, though it must also be said that she was not an easy colleague; but neither are many male academics. That colleagues chose to ignore her rather than make the effort to understand what she was saying suggests that they may not have taken her as seriously as they would have taken a male colleague of comparable experience and stature. The degree to which McClintock was something of an outsider and a loner in her scientific life (though she always had close friends) probably also had something to do with gender. But the content of her science?

Some people have suggested that McClintock relied more on intuition than do most male scientists. Probably so, but so do some men. In a recent biography of Henry Wallace, Franklin Roosevelt's two-term sec-

retary of agriculture and one-term vice president, who was a world-famous plant breeder (and founder of Pioneer-Hi-Bred, the foremost supplier of hybrid corn), I found the following story. Late in Wallace's life, a group of New York writers and artists asked him to what he attributed his success as a plant breeder. Wallace responded, "Sympathy with the plant" (Culver and Hyde 2000, 518), quite like McClintock's "feeling for the organism," her phrase that Evelyn Fox Keller uses as the title for her biography.

IV

Rosalind Franklin's is a much sadder story. Franklin was born in 1920 into an established Anglo-Jewish family in London. She graduated from Cambridge University during World War II with a degree in physical chemistry and went to work doing war-related research on different configurations of carbon in coal. At the end of the war, she moved to Paris and took a position in a French government laboratory, using X-ray diffraction techniques to analyze the structure of different types of coal. After four happy years there, she reluctantly decided to return to England and, because she wanted to learn about molecules of biological interest, accepted a fellowship in the biophysics unit at King's College, London, directed by Professor John Randall. The unit was working on the structure of DNA, and Randall asked Franklin to build a high-resolution camera with which to make more detailed measurements of the X-ray diffraction patterns of DNA than had previously been possible.³

King's was a much less collegial and more hierarchical place than the laboratory in which Franklin had been working in Paris, with gender-segregated "combination rooms" where the staff took their tea and morning coffee. Also, intentionally or not, Randall put Franklin into a highly ambiguous situation by leading Maurice Wilkins, the unit's assistant director, to believe that Franklin and he would be working on DNA together, while telling Franklin she would be doing the X-ray diffraction studies on her own.⁴ When personality conflicts began to develop between Franklin and Wilkins, she decided they would not be able to work together

³ I draw my information about Franklin from Brenda Maddox's recent biography *Rosalind Franklin: The Dark Lady of DNA* (2002); from Anne Sayre's *Rosalind Franklin and DNA* (1975), which Sayre wrote as a much-needed corrective to James D. Watson's *The Double Helix* (1968); from Watson's book itself; and from the historian of science Robert Olby's *The Path to the Double Helix* (1974). I have also consulted some of Franklin's own publications and articles colleagues and friends have written about her and have spoken with some of her friends, including Sayre.

⁴ Maddox 2002, 132–33.

and set about to build a powerful X-ray camera with which she and Wilkins's former graduate student R. G. Gosling began to make a series of groundbreaking observations on DNA fibers. And before long, she obtained the sharpest X-ray diffraction image of DNA in existence that clearly showed that DNA can form a helix.

Shortly after Franklin joined the group at King's, James Watson came to Cambridge University planning to work with Francis Crick on the structure of DNA, which the two of them considered to be "the secret of life." As Watson recounts in *The Double Helix*, his first encounter with Franklin was a disaster. Soon after coming to Cambridge, Watson went to King's to attend a seminar by Franklin, but he was too busy critiquing her clothes and hairstyle to listen properly. Having misunderstood her presentation, he told what he remembered of it to Crick, and they promptly decided to use his recollection to build a model of DNA. They then invited the King's group to come and look at their model. Franklin immediately realized that it was completely inconsistent with the data she had presented at the seminar Watson had attended and decided he was not to be taken seriously. In consequence of this fiasco, their superiors at Cambridge told Watson and Crick to keep their hands off DNA and leave it to the group at King's. (Watson tells all this in the *The Double Helix*.)

During the next months, unbeknownst to Franklin, two crucial things happened. One was that Wilkins showed Watson Franklin's best X-ray diffraction image, which clearly indicated that DNA forms a helix. The other was that Max Perutz, a senior researcher at Cambridge, received a research report the King's group had submitted to their funders. Knowing of Watson's and Crick's interest in DNA, he showed them the report, which included the conclusions Franklin had drawn on the basis of her X-ray image, conclusions that specified all the critical dimensions of the DNA helix.

At just about this time, Watson and Crick found out that the famous U.S. chemist Linus Pauling was about to propose a transparently incorrect structure for DNA. With that, they decided they no longer needed to consider DNA the property of King's. Armed with Franklin's calculations against which to check possible models, they went into a frenzy of model building and, within about six weeks, came up with the now-famous double helix.

The first time the group at King's realized that Watson and Crick had gone back to working on DNA was when Wilkins received in the mail a copy of the note Watson and Crick were submitting to *Nature*. He promptly decided to write an accompanying note with his coworkers Stokes and Wilson, and so did Franklin with her coworker Gosling. Frank-

lin framed her note as though her data confirmed the Watson-Crick structure, since she had no idea that those data had been in their hands while they puzzled out the structure. And she never realized it because five years later, in 1958, at thirty-seven years old, she died of cancer. She was dead when Watson, Crick, and Wilkins shared the Nobel Prize in 1962 and, of course, when Watson published *The Double Helix* in 1968.

In fact, Watson could never have published that book had Franklin been alive. In addition to the personal jabs and the book's crude sexism, until Watson wrote *The Double Helix* only he and Crick knew that they were in possession of Franklin's calculations while they constructed their model. Clearly, except for Franklin's closest friends, the book's readers continued to overlook that fact. But certainly Franklin would have noticed!

As it was, in 1953, when the three *Nature* papers appeared in print, Franklin was in the process of moving from King's to the much more collegial laboratory of J. D. Bernal at Birkbeck College, which is where she spent her few remaining years doing outstanding work on the structure of viruses.

So, again, how does gender come into this story? Gender no doubt had something to do with Franklin's unsatisfactory experience at King's. It probably also had something to do with the way Watson and Crick dealt with her data (though the misappropriation of data need not involve gender). It perhaps also was at least partly responsible for Franklin's lowly academic status, though she was still young and, at the time of her death, was in the midst of negotiating a move to a research position with secure, long-term funding at Cambridge University for herself and her principal collaborators at Birkbeck.⁶

Perhaps the most interesting aspect in terms of gender politics, however, is the way Watson used sexist stereotypes to obscure what should have become a scientific scandal. As Franklin's friend Anne Sayre recognized the moment she began to read *The Double Helix*, the creation of "Rosy," the humorless, dowdy, castrating female who, rather than help her dedicated male "superior" Wilkins, as she was meant to do, insists on imposing her own ideas, has the function of getting the reader not to notice that Watson and Crick had access to Franklin's unpublished data while they made their biology-shaking discovery.

If not for the fact that Franklin had long since died, Watson could not have written that story the way he did—or, more likely, at all. That he wrote it and that his breezy description of the way he and Crick came to the double helix succeeded in burying the unsavory details can surely be

⁶ Maddox 2002, 304–5.

attributed to sexual politics. But, as I have argued before, gender was not an issue in Franklin's science any more than it was in McClintock's.

That McClintock's science was highly individual is clear. Indeed, some have argued that her scientific iconoclasm was not unrelated to the apparent comfort she took in her outsider status, which must have at least partly had to do with being a nontraditional woman. Also, Franklin's work was probably influenced by her exclusion from the King's/Cambridge fraternity, though she, too, did not let that stop her. Other women, in addition to McClintock and Franklin, have been at the forefront of genetics and molecular biology. The fact that news stories about this highly publicized field usually feature male scientists (and especially Watson) simply illustrates the gender politics of our culture.

All this is not to say that being a woman or a man is irrelevant to the way one does science. No doubt, our experiences affect what aspects of the world interest us and how we come to think about them, but ovaries or testes do not directly affect what science we do and how we do it.

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Posthumanist Performativity: Toward an Understanding of How Matter Comes to Matter

Where did we ever get the strange idea that nature—as opposed to culture—is ahistorical and timeless? We are far too impressed by our own cleverness and self-consciousness. . . . We need to stop telling ourselves the same old anthropocentric bedtime stories.

—Steve Shaviro 1997

Language has been granted too much power. The linguistic turn, the semiotic turn, the interpretative turn, the cultural turn: it seems that at every turn lately every “thing”—even materiality—is turned into a matter of language or some other form of cultural representation. The ubiquitous puns on “matter” do not, alas, mark a rethinking of the key concepts (materiality and signification) and the relationship between them. Rather, it seems to be symptomatic of the extent to which matters of “fact” (so to speak) have been replaced with matters of signification (no scare quotes here). Language matters. Discourse matters. Culture matters. There is an important sense in which the only thing that does not seem to matter anymore is matter.

What compels the belief that we have a direct access to cultural representations and their content that we lack toward the things represented? How did language come to be more trustworthy than matter? Why are language and culture granted their own agency and historicity while matter is figured as passive and immutable, or at best inherits a potential for change derivatively from language and culture? How does one even go about inquiring after the material conditions that have led us to such a brute reversal of naturalist beliefs when materiality itself is always already figured within a linguistic domain as its condition of possibility?

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It is hard to deny that the power of language has been substantial. One might argue too substantial, or perhaps more to the point, too substantializing. Neither an exaggerated faith in the power of language nor the expressed concern that language is being granted too much power is a novel apprehension specifically attached to the early twenty-first century. For example, during the nineteenth century Nietzsche warned against the mistaken tendency to take grammar too seriously: allowing linguistic structure to shape or determine our understanding of the world, believing that the subject and predicate structure of language reflects a prior ontological reality of substance and attribute. The belief that grammatical categories reflect the underlying structure of the world is a continuing seductive habit of mind worth questioning. Indeed, the representationalist belief in the power of words to mirror preexisting phenomena is the metaphysical substrate that supports social constructivist, as well as traditional realist, beliefs. Significantly, social constructivism has been the object of intense scrutiny within both feminist and science studies circles where considerable and informed dissatisfaction has been voiced.¹

A *performative* understanding of discursive practices challenges the representationalist belief in the power of words to represent preexisting things. Performativity, properly construed, is not an invitation to turn everything (including material bodies) into words; on the contrary, performativity is precisely a contestation of the excessive power granted to language to determine what is real. Hence, in ironic contrast to the misconception that would equate performativity with a form of linguistic monism that takes language to be the stuff of reality, performativity is actually a contestation of the unexamined habits of mind that grant language and other forms of representation more power in determining our ontologies than they deserve.²

The move toward performative alternatives to representationalism shifts the focus from questions of correspondence between descriptions and reality (e.g., do they mirror nature or culture?) to matters of practices/ doings/actions. I would argue that these approaches also bring to the forefront important questions of ontology, materiality, and agency, while social constructivist approaches get caught up in the geometrical optics

¹ Dissatisfaction surfaces in the literature in the 1980s. See, e.g., Donna Haraway's "Gender for a Marxist Dictionary: The Sexual Politics of a Word" (originally published 1987) and "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective" (originally published 1988); both reprinted in Haraway 1991. See also Butler 1989.

² This is not to dismiss the valid concern that certain specific performative accounts grant too much power to language. Rather, the point is that this is not an inherent feature of performativity but an ironic malady.

of reflection where, much like the infinite play of images between two facing mirrors, the epistemological gets bounced back and forth, but nothing more is seen. Moving away from the representationalist trap of geometrical optics, I shift the focus to physical optics, to questions of diffraction rather than reflection. Diffractively reading the insights of feminist and queer theory and science studies approaches through one another entails thinking the "social" and the "scientific" together in an illuminating way. What often appears as separate entities (and separate sets of concerns) with sharp edges does not actually entail a relation of absolute exteriority at all. Like the diffraction patterns illuminating the indefinite nature of boundaries—displaying shadows in "light" regions and bright spots in "dark" regions—the relation of the social and the scientific is a relation of "exteriority within." This is not a static relationality but a doing—the enactment of boundaries—that always entails constitutive exclusions and therefore requisite questions of accountability.³ My aim is to contribute to efforts to sharpen the theoretical tool of performativity for science studies and feminist and queer theory endeavors alike, and to promote their mutual consideration. In this article, I offer an elaboration of performativity—a materialist, naturalist, and posthumanist elaboration—that allows matter its due as an active participant in the world's becoming, in its ongoing "intra-activity."⁴ It is vitally important that we understand how matter matters.

From representationalism to performativity

People represent. That is part of what it is to be a person. . . . Not *homo faber*, I say, but *homo depictor*.

—Ian Hacking 1983, 144, 132

Liberal social theories and theories of scientific knowledge alike owe much to the idea that the world is composed of individuals—presumed to exist

³ Haraway proposes the notion of diffraction as a metaphor for rethinking the geometry and optics of relationality. "[F]eminist theorist Trinh Minh-ha . . . was looking for a way to figure 'difference' as a 'critical difference within,' and not as special taxonomic marks grounding difference as apartheid. . . . Diffraction does not produce 'the same' displaced, as reflection and refraction do. Diffraction is a mapping of interference, not of replication, reflection, or reproduction. A diffraction pattern does not map where differences appear, but rather maps where the *effects* of differences appear" (1992, 300). Haraway (1997) promotes the notion of diffraction to a fourth semiotic category. Inspired by her suggestions for usefully deploying this rich and fascinating physical phenomenon to think about differences that matter, I further elaborate the notion of diffraction as a mutated critical tool of analysis (though not as a fourth semiotic category) in my forthcoming book (Barad forthcoming).

⁴ See Rouse 2002 on rethinking naturalism. The neologism *intra-activity* is defined below.

before the law, or the discovery of the law—awaiting/inviting representation. The idea that beings exist as individuals with inherent attributes, anterior to their representation, is a metaphysical presupposition that underlies the belief in political, linguistic, and epistemological forms of representationalism. Or, to put the point the other way around, representationalism is the belief in the ontological distinction between representations and that which they purport to represent; in particular, that which is represented is held to be independent of all practices of representing. That is, there are assumed to be two distinct and independent kinds of entities—representations and entities to be represented. The system of representation is sometimes explicitly theorized in terms of a tripartite arrangement. For example, in addition to knowledge (i.e., representations), on the one hand, and the known (i.e., that which is purportedly represented), on the other, the existence of a knower (i.e., someone who does the representing) is sometimes made explicit. When this happens it becomes clear that representations serve a mediating function between independently existing entities. This taken-for-granted ontological gap generates questions of the accuracy of representations. For example, does scientific knowledge accurately represent an independently existing reality? Does language accurately represent its referent? Does a given political representative, legal counsel, or piece of legislation accurately represent the interests of the people allegedly represented?

Representationalism has received significant challenge from feminists, poststructuralists, postcolonial critics, and queer theorists. The names of Michel Foucault and Judith Butler are frequently associated with such questioning. Butler sums up the problematics of political representationism as follows:

Foucault points out that juridical systems of power *produce* the subjects they subsequently come to represent. Juridical notions of power appear to regulate political life in purely negative terms. . . . But the subjects regulated by such structures are, by virtue of being subjected to them, formed, defined, and reproduced in accordance with the requirements of those structures. If this analysis is right, then the juridical formation of language and politics that represents women as “the subject” of feminism is itself a discursive formation and effect of a given version of representationalist politics. And the feminist subject turns out to be discursively constituted by the very political system that is supposed to facilitate its emancipation. (1990, 2)

In an attempt to remedy this difficulty, critical social theorists struggle to formulate understandings of the possibilities for political intervention that go beyond the framework of representationalism.

The fact that representationalism has come under suspicion in the domain of science studies is less well known but of no less significance. Critical examination of representationalism did not emerge until the study of science shifted its focus from the nature and production of scientific knowledge to the study of the detailed dynamics of the actual practice of science. This significant shift is one way to coarsely characterize the difference in emphasis between separate multiple disciplinary studies of science (e.g., history of science, philosophy of science, sociology of science) and science studies. This is not to say that all science studies approaches are critical of representationalism; many such studies accept representationalism unquestioningly. For example, there are countless studies on the nature of scientific representations (including how scientists produce them, interpret them, and otherwise make use of them) that take for granted the underlying philosophical viewpoint that gives way to this focus—namely, representationalism. On the other hand, there has been a concerted effort by some science studies researchers to move beyond representationalism.

Ian Hacking's *Representing and Intervening* (1983) brought the question of the limitations of representationalist thinking about the nature of science to the forefront. The most sustained and thoroughgoing critique of representationalism in philosophy of science and science studies is to be found in the work of philosopher of science Joseph Rouse. Rouse has taken the lead in interrogating the constraints that representationalist thinking places on theorizing the nature of scientific practices.⁶ For example, while the hackneyed debate between scientific realism and social constructivism moved frictionlessly from philosophy of science to science studies, Rouse (1996) has pointed out that these adversarial positions have more in common than their proponents acknowledge. Indeed, they share representationalist assumptions that foster such endless debates: both scientific realists and social constructivists believe that scientific knowledge (in its multiple representational forms such as theoretical concepts, graphs,

⁶ Rouse begins his interrogation of representationalism in *Knowledge and Power* (1987). He examines how a representationalist understanding of knowledge gets in the way of understanding the nature of the relationship between power and knowledge. He continues his critique of representationalism and the development of an alternative understanding of the nature of scientific practices in *Engaging Science* (1996). Rouse proposes that we understand science practice as ongoing patterns of situated activity, an idea that is then further elaborated in *How Scientific Practices Matter* (2002).

particle tracks, photographic images) mediates our access to the material world; where they differ is on the question of referent, whether scientific knowledge represents things in the world as they really are (i.e., "Nature") or "objects" that are the product of social activities (i.e., "Culture"), but both groups subscribe to representationalism.

Representationalism is so deeply entrenched within Western culture that it has taken on a commonsense appeal. It seems inescapable, if not downright natural. But representationalism (like "nature itself," not merely our representations of it!) has a history. Hacking traces the philosophical problem of representations to the Democritean dream of atoms and the void. According to Hacking's anthropological philosophy, representations were unproblematic prior to Democritus: "the word 'real' first meant just unqualified likeness" (142). With Democritus's atomic theory emerges the possibility of a gap between representations and represented—"appearance" makes its first appearance. Is the table a solid mass made of wood or an aggregate of discrete entities moving in the void? Atomism poses the question of which representation is real. The problem of realism in philosophy is a product of the atomistic worldview.

Rouse identifies representationalism as a Cartesian by-product—a particularly inconspicuous consequence of the Cartesian division between "internal" and "external" that breaks along the line of the knowing subject. Rouse brings to light the asymmetrical faith in word over world that underlines the nature of Cartesian doubt:

I want to encourage doubt about [the] presumption that representations (that is, their meaning or content) are more accessible to us than the things they supposedly represent. If there is no magic language through which we can unerringly reach out directly to its referents, why should we think there is nevertheless a language that magically enables us to reach out directly to its sense or representational content? The presumption that we can know what we mean, or what our verbal performances say, more readily than we can know the objects those sayings are about is a Cartesian legacy, a linguistic variation on Descartes' insistence that we have a direct and privileged access to the contents of our thoughts that we lack towards the "external" world. (1996, 209)

In other words, the asymmetrical faith in our access to representations over things is a contingent fact of history and not a logical necessity; that

is, it is simply a Cartesian habit of mind. It takes a healthy skepticism toward Cartesian doubt to be able to begin to see an alternative.⁶

Indeed, it is possible to develop coherent philosophical positions that deny that there are representations on the one hand and ontologically separate entities awaiting representation on the other. A performative understanding, which shifts the focus from linguistic representations to discursive practices, is one such alternative. In particular, the search for alternatives to social constructivism has prompted performative approaches in feminist and queer studies, as well as in science studies. Judith Butler's name is most often associated with the term *performativity* in feminist and queer theory circles. And while Andrew Pickering has been one of the very few science studies scholars to take ownership of this term, there is surely a sense in which science studies theorists such as Donna Haraway, Bruno Latour, and Joseph Rouse also propound performative understandings of the nature of scientific practices.⁷ Indeed, *performativity* has become a ubiquitous term in literary studies, theater studies, and the nascent interdisciplinary area of performance studies, prompting the question as

⁶ The allure of representationalism may make it difficult to imagine alternatives. I discuss performative alternatives below, but these are not the only ones. A concrete historical example may be helpful at this juncture. Foucault points out that in sixteenth-century Europe, language was not thought of as a medium; rather, it was simply "one of the figurations of the world" (1970, 56), an idea that reverberates in a mutated form in the posthumanist performative account that I offer.

⁷ Andrew Pickering (1995) explicitly eschews the representationalist idiom in favor of a performative idiom. It is important to note, however, that Pickering's notion of performativity would not be recognizable as such to poststructuralists, despite their shared embrace of *performativity* as a remedy to representationalism, and despite their shared rejection of humanism. Pickering's appropriation of the term does not include any acknowledgement of its politically important—arguably inherently queer—genealogy (see Sedgwick 1993) or why it has been and continues to be important to contemporary critical theorists, especially feminist and queer studies scholars/activists. Indeed, he evacuates its important political historicity along with many of its crucial insights. In particular, Pickering ignores important discursive dimensions, including questions of meaning, intelligibility, significance, identity formation, and power, which are central to poststructuralist invocations of "performativity." And he takes for granted the humanist notion of agency as a *property* of individual entities (such as humans, but also weather systems, scallops, and stereotypes), which poststructuralists problematize. On the other hand, poststructuralist approaches fail to take account of "nonhuman agency," which is a central focus of Pickering's account. See Barad (forthcoming) for a more detailed discussion.

to whether all performances are performative.⁸ In this article, I propose a specifically posthumanist notion of performativity—one that incorporates important material and discursive, social and scientific, human and nonhuman, and natural and cultural factors. A posthumanist account calls into question the givenness of the differential categories of “human” and “nonhuman,” examining the practices through which these differential boundaries are stabilized and destabilized.⁹ Donna Haraway’s scholarly opus—from primates to cyborgs to companion species—epitomizes this point.

If performativity is linked not only to the formation of the subject but also to the production of the matter of bodies, as Butler’s account of “materialization” and Haraway’s notion of “materialized refiguration” suggest, then it is all the more important that we understand the nature of this production.¹⁰ Foucault’s analytic of power links discursive practices to the materiality of the body. However, his account is constrained by several important factors that severely limit the potential of his analysis and Butler’s performative elaboration, thereby forestalling an understanding of precisely *how* discursive practices produce material bodies.

⁸ The notion of performativity has a distinguished career in philosophy that most of these multiple and various engagements acknowledge. Performativity’s lineage is generally traced to the British philosopher J. L. Austin’s interest in speech acts, particularly the relationship between saying and doing. Jacques Derrida is usually cited next as offering important poststructuralist amendments. Butler elaborates Derrida’s notion of performativity through Foucault’s understanding of the productive effects of regulatory power in theorizing the notion of identity performatively. Butler introduces her notion of gender performativity in *Gender Trouble*, where she proposes that we understand gender not as a thing or a set of free-floating attributes, not as an essence—but rather as a “doing”: “gender is itself a kind of becoming or activity . . . gender ought not to be conceived as a noun or a substantial thing or a static cultural marker, but rather as an incessant and repeated action of some sort” (1990, 112). In *Bodies That Matter* (1993) Butler argues for a linkage between gender performativity and the materialization of sexed bodies. Eve Kosofsky Sedgwick (1993) argues that performativity’s genealogy is inherently queer.

⁹ This notion of posthumanism differs from Pickering’s idiosyncratic assignment of a “posthumanist space [as] a space in which the human actors are still there but now inextricably entangled with the nonhuman, no longer at the center of the action calling the shots” (26). However, the decentering of the human is but one element of posthumanism. (Note that Pickering’s notion of “entanglement” is explicitly epistemological, not ontological. What is at issue for him in dubbing his account “posthumanist” is the fact that it is attentive to the mutual accommodation, or responsiveness, of human and nonhuman agents.)

¹⁰ It could be argued that “materialized refiguration” is an *entirely up* (Haraway’s term) version of “materialization,” while the notion of “materialization” hints at a richer account of the former. Indeed, it is possible to read my posthumanist performative account along these lines, as a diffractive elaboration of Butler’s and Haraway’s crucial insights.

If Foucault, in queering Marx, positions the body as the locus of productive forces, the site where the large-scale organization of power links up with local practices, then it would seem that any robust theory of the materialization of bodies would necessarily take account of *how the body's materiality*—for example, its anatomy and physiology—*and other material forces actively matter to the processes of materialization*. Indeed, as Foucault makes crystal clear in the last chapter of *The History of Sexuality* (vol. 1), he is not out to deny the relevance of the physical body but, on the contrary, to

show how the deployments of power are directly connected to the body—to bodies, functions, physiological processes, sensations, and pleasures; far from the body having to be effaced, what is needed is to make it visible through an analysis in which the biological and the historical are not consecutive to one another . . . but are bound together in an increasingly complex fashion in accordance with the development of the modern technologies of power that take life as their objective. Hence, I do not envision a “history of mentalities” that would take account of bodies only through the manner in which they have been perceived and given meaning and value; but a “history of bodies” and the manner in which what is most material and most vital in them has been invested. (1980a, 151–52)

On the other hand, Foucault does not tell us in what way the biological and the historical are “bound together” such that one is not consecutive to the other. What is it about the materiality of bodies that makes it susceptible to the enactment of biological and historical forces simultaneously? To what degree does the matter of bodies have its own historicity? Are social forces the only ones susceptible to change? Are not biological forces in some sense always already historical ones? Could it be that there is some important sense in which historical forces are always already biological? What would it mean to even ask such a question given the strong social constructivist undercurrent in certain interdisciplinary circles in the early twenty-first century? For all Foucault's emphasis on the political anatomy of disciplinary power, he too fails to offer an account of the body's historicity in which its very materiality plays an *active* role in the workings of power. This implicit reinscription of matter's passivity is a mark of extant elements of representationalism that haunt his largely post-representationalist account.¹¹ This deficiency is importantly related to his failure to theorize the relationship between “discursive” and “nondiscur-

¹¹ See also Butler 1989.

sive" practices. As materialist feminist theorist Rosemary Hennessey insists in offering her critique of Foucault, "a rigorous materialist theory of the body cannot stop with the assertion that the body is always discursively constructed. It also needs to explain how the discursive construction of the body is related to nondiscursive practices in ways that vary widely from one social formation to another" (1993, 46).

Crucial to understanding the workings of power is an understanding of the nature of power in the fullness of its materiality. To restrict power's productivity to the limited domain of the "social," for example, or to figure matter as merely an end product rather than an active factor in further materializations, is to cheat matter out of the fullness of its capacity. How might we understand not only how human bodily contours are constituted through psychic processes but how even the very atoms that make up the biological body come to matter and, more generally, how matter makes itself felt? It is difficult to imagine how psychic and socio-historical forces alone could account for the production of matter. Surely it is the case—even when the focus is restricted to the materiality of "human" bodies—that there are "natural," not merely "social," forces that matter. Indeed, there is a host of material-discursive forces—including ones that get labeled "social," "cultural," "psychic," "economic," "natural," "physical," "biological," "geopolitical," and "geological"—that may be important to particular (entangled) processes of materialization. If we follow disciplinary habits of tracing disciplinary-defined causes through to the corresponding disciplinary-defined effects, we will miss all the crucial intra-actions among these forces that fly in the face of any specific set of disciplinary concerns.¹²

What is needed is a robust account of the materialization of *all* bodies—"human" and "nonhuman"—and the material-discursive practices by which their differential constitutions are marked. This will require an understanding of the nature of the relationship between discursive practices and material phenomena, an accounting of "nonhuman" as well as "human" forms of agency, and an understanding of the precise causal nature of productive practices that takes account of the fullness of matter's implication in its ongoing historicity. My contribution toward the development of such an understanding is based on a philosophical account that I have been calling "agential realism." Agential realism is an account of technoscientific and other practices that takes feminist, antiracist, post-structuralist, queer, Marxist, science studies, and scientific insights seri-

¹² The conjunctive term *material-discursive* and other agential realist terms like *intra-action* are defined below.

ously, building specifically on important insights from Niels Bohr, Judith Butler, Michel Foucault, Donna Haraway, Vicki Kirby, Joseph Rouse, and others.¹⁸ It is clearly not possible to fully explicate these ideas here. My more limited goal in this article is to use the notion of performativity as a diffraction grating for reading important insights from feminist and queer studies and science studies through one another while simultaneously proposing a materialist and posthumanist reworking of the notion of performativity. This entails a reworking of the familiar notions of discursive practices, materialization, agency, and causality, among others.

I begin by issuing a direct challenge to the metaphysical underpinnings of representationalism, proposing an agential realist ontology as an alternative. In the following section I offer a posthumanist performative reformulation of the notion of discursive practices and materiality and theorize a specific causal relationship between them. In the final section I discuss the agential realist conceptions of causality and agency that are vital to understanding the productive nature of material-discursive practices, including technoscientific ones.

Toward a performative metaphysics

As long as we stick to things and words we can believe that we are speaking of what we see, that we see what we are speaking of, and that the two are linked.

—Gilles Deleuze 1988, 65

"Words and things" is the entirely serious title of a problem.

— Michel Foucault 1972, 49

Representationalism separates the world into the ontologically disjoint domains of words and things, leaving itself with the dilemma of their linkage such that knowledge is possible. If words are untethered from the material world, how do representations gain a foothold? If we no longer believe that the world is teeming with inherent resemblances whose signatures are inscribed on the face of the world, things already emblazoned with signs, words lying in wait like so many pebbles of sand on a beach there to be discovered, but rather that the knowing subject is enmeshed in a thick web of representations such that the mind cannot see its way

¹⁸ This essay outlines issues I developed in earlier publications including Barad 1996, 1998a, 1998b, 2001b, and in my forthcoming book (Barad forthcoming).

to objects that are now forever out of reach and all that is visible is the sticky problem of humanity's own captivity within language, then it begins to become apparent that representationalism is a prisoner of the problematic metaphysics it postulates. Like the frustrated would-be runner in Zeno's paradox, representationalism never seems to be able to get any closer to solving the problem it poses because it is caught in the impossibility of stepping outward from its metaphysical starting place. Perhaps it would be better to begin with a different starting point, a different metaphysics.¹⁴

Thingification—the turning of relations into “things,” “entities,” “relata”—infects much of the way we understand the world and our relationship to it.¹⁵ Why do we think that the existence of relations requires relata? Does the persistent distrust of nature, materiality, and the body that pervades much of contemporary theorizing and a sizable amount of the history of Western thought feed off of this cultural proclivity? In this section, I present a relational ontology that rejects the metaphysics of relata, of “words” and “things.” On an agential realist account, it is once again possible to acknowledge nature, the body, and materiality in the fullness of their becoming without resorting to the optics of transparency or opacity, the geometries of absolute exteriority or interiority, and the theoretization of the human as either pure cause or pure effect while at the same time remaining resolutely accountable for the role “we” play in the intertwined practices of knowing and becoming.

The postulation of individually determinate entities with inherent properties is the hallmark of atomistic metaphysics. Atomism hails from Democritus.¹⁶ According to Democritus the properties of all things derive

¹⁴ It is no secret that *metaphysics* has been a term of opprobrium through most of the twentieth century. This positivist legacy lives on even in the heart of its detractors. Post-structuralists are simply the newest signatories of its death warrant. Yet, however strong one's dislike of metaphysics, it will not abide by any death sentence, and so it is ignored at one's peril. Indeed, new “experimental metaphysics” research is taking place in physics laboratories in the United States and abroad, calling into question the common belief that there is an inherent boundary between the “physical” and the “metaphysical” (see Barad forthcoming). This fact should not be too surprising to those of us who remember that the term *metaphysics* does not have some highbrow origins in the history of philosophy but, rather, originally referred to the writings of Aristotle that came after his writings on physics, in the arrangement made by Andronicus of Rhodes about three centuries after Aristotle's death.

¹⁵ *Relata* are would-be antecedent components of relations. According to metaphysical atomism, individual relata always preexist any relations that may hold between them.

¹⁶ Atomism is said to have originated with Leucippus and was further elaborated by Democritus, devotee of democracy, who also explored its anthropological and ethical implications. Democritus's atomic theory is often identified as the most mature pre-Socratic

from the properties of the smallest unit—atoms (the “uncuttable” or “inseparable”). Liberal social theories and scientific theories alike owe much to the idea that the world is composed of individuals with separately attributable properties. An entangled web of scientific, social, ethical, and political practices, and our understanding of them, hinges on the various/differential instantiations of this presupposition. Much hangs in the balance in contesting its seeming inevitability.

Physicist Niels Bohr won the Nobel Prize for his quantum model of the atom, which marks the beginning of his seminal contributions to the development of the quantum theory.¹⁷ Bohr’s philosophy-physics (the two were inseparable for him) poses a radical challenge not only to Newtonian physics but also to Cartesian epistemology and its representationalist triadic structure of words, knowers, and things. Crucially, in a stunning reversal of his intellectual forefather’s schema, Bohr rejects the atomistic metaphysics that takes “things” as ontologically basic entities. For Bohr, things do not have inherently determinate boundaries or properties, and words do not have inherently determinate meanings. Bohr also calls into question the related Cartesian belief in the inherent distinction between subject and object, and knower and known.

It might be said that the epistemological framework that Bohr develops rejects both the transparency of language and the transparency of measurement; however, even more fundamentally, it rejects the presupposition that language and measurement perform mediating functions. Language does not represent states of affairs, and measurements do not represent measurement-independent states of being. Bohr develops his epistemological framework without giving in to the despair of nihilism or the sticky web of relativism. With brilliance and finesse, Bohr finds a way to hold on to the possibility of objective knowledge while the grand structures of Newtonian physics and representationalism begin to crumble.

Bohr’s break with Newton, Descartes, and Democritus is not based in “mere idle philosophical reflection” but on new empirical findings in the domain of atomic physics that came to light during the first quarter of the twentieth century. Bohr’s struggle to provide a theoretical under-

philosophy, directly influencing Plato and Epicurus, who transmitted it into the early modern period. Atomic theory is also said to form the cornerstone of modern science.

¹⁷ Niels Bohr (1885–1962), a contemporary of Einstein, was one of the founders of quantum physics and also the most widely accepted interpretation of the quantum theory, which goes by the name of the Copenhagen interpretation (after the home of Bohr’s internationally acclaimed physics institute that bears his name). On my reading of Bohr’s philosophy-physics, Bohr can be understood as proposing a protoperformative account of scientific practices.

standing of these findings resulted in his radical proposal that an entirely new epistemological framework is required. Unfortunately, Bohr does not explore crucial ontological dimensions of his insights but rather focuses on their epistemological import. I have mined his writings for his implicit ontological views and have elaborated on them in the development of an agential realist ontology. In this section, I present a quick overview of important aspects of Bohr's account and move on to an explication of an agential realist ontology. This relational ontology is the basis for my post-humanist performative account of the production of material bodies. This account refuses the representationalist fixation on "words" and "things" and the problematic of their relationality, advocating instead *a causal relationship between specific exclusionary practices embodied as specific material configurations of the world* (i.e., discursive practices/(con)figurations rather than "words") *and specific material phenomena* (i.e., relations rather than "things"). This causal relationship between the apparatuses of bodily production and the phenomena produced is one of "agential intra-action." The details follow.

According to Bohr, *theoretical concepts* (e.g., "position" and "momentum") are not ideational in character but rather *are specific physical arrangements*.¹⁸ For example, the notion of "position" cannot be presumed to be a well-defined abstract concept, nor can it be presumed to be an inherent attribute of independently existing objects. Rather, "position" only has meaning when a rigid apparatus with fixed parts is used (e.g., a ruler is nailed to a fixed table in the laboratory, thereby establishing a fixed frame of reference for specifying "position"). And furthermore, any measurement of "position" using this apparatus cannot be attributed to some abstract independently existing "object" but rather is a property of the *phenomenon*—the inseparability of "observed object" and "agencies of observation." Similarly, "momentum" is only meaningful as a material arrangement involving movable parts. Hence, the simultaneous indeterminacy of "position" and "momentum" (what is commonly referred to as the Heisenberg uncertainty principle) is a straightforward matter of the material exclusion of "position" and "momentum" arrangements (one requiring fixed parts and the complementary arrangement requiring movable parts).¹⁹

¹⁸ Bohr argues on the basis of this single crucial insight, together with the empirical finding of an inherent discontinuity in measurement "intra-actions," that one must reject the presumed inherent separability of observer and observed, knower and known. See Barad 1996, forthcoming.

¹⁹ The so-called uncertainty principle in quantum physics is not a matter of "uncertainty" at all but rather of indeterminacy. See Barad 1995, 1996, forthcoming.

Therefore, according to Bohr, the primary epistemological unit is not independent objects with inherent boundaries and properties but rather *phenomena*. On my agential realist elaboration, phenomena do not merely mark the epistemological inseparability of "observer" and "observed"; rather, *phenomena are the ontological inseparability of agentially intra-acting "components."* That is, phenomena are ontologically primitive relations—relations without preexisting relata.²⁰ The notion of *intra-action* (in contrast to the usual "interaction," which presumes the prior existence of independent entities/relata) represents a profound conceptual shift. It is through specific agential intra-actions that the boundaries and properties of the "components" of phenomena become determinate and that particular embodied concepts become meaningful. A specific intra-action (involving a specific material configuration of the "apparatus of observation") enacts an *agential cut* (in contrast to the Cartesian cut—an inherent distinction—between subject and object) effecting a separation between "subject" and "object." That is, the agential cut enacts a *local resolution within* the phenomenon of the inherent ontological indeterminacy. In other words, relata do not preexist relations; rather, relata-within-phenomena emerge through specific intra-actions. Crucially then, intra-actions enact *agential separability*—the local condition of *exteriority-within-phenomena*. The notion of agential separability is of fundamental importance, for in the absence of a classical ontological condition of exteriority between observer and observed it provides the condition for the possibility of objectivity. Moreover, the agential cut enacts a local causal structure among "components" of a phenomenon in the marking of the "measuring agencies" ("effect") by the "measured object" ("cause"). Hence, *the notion of intra-actions constitutes a reworking of the traditional notion of causality.*²¹

²⁰ That is, relations are not secondarily derived from independently existing "relata," but rather the mutual ontological dependence of "relata"—the relation—is the ontological primitive. As discussed below, relata only exist *within* phenomena as a result of specific intra-actions (i.e., there are no independent relata, only relata-within-relations).

²¹ A concrete example may be helpful. When light passes through a two-slit diffraction grating and forms a diffraction pattern it is said to exhibit wavelike behavior. But there is also evidence that light exhibits particlelike characteristics, called *photons*. If one wanted to test this hypothesis, the diffraction apparatus could be modified in such a way as to allow a determination of which slit a given photon passes through (since particles only go through a single slit at a time). The result of running this experiment is that the diffraction pattern is destroyed! Classically, these two results together seem contradictory—frustrating efforts to specify the true ontological nature of light. Bohr resolves this wave-particle duality paradox as follows: the objective referent is not some abstract, independently existing entity but rather the phenomenon of light intra-acting with the apparatus. The first apparatus gives determinate

In my further elaboration of this agential realist ontology, I argue that phenomena are not the mere result of laboratory exercises engineered by human subjects. Nor can the apparatuses that produce phenomena be understood as observational devices or mere laboratory instruments. Although space constraints do not allow an in-depth discussion of the agential realist understanding of the nature of apparatuses, since apparatuses play such a crucial, indeed constitutive, role in the production of phenomena, I present an overview of the agential realist theoretization of apparatuses before moving on to the question of the nature of phenomena. The proposed elaboration enables an exploration of the implications of the agential realist ontology beyond those specific to understanding the nature of scientific practices. In fact, agential realism offers an understanding of the nature of material-discursive practices, such as those very practices through which different distinctions get drawn, including those between the "social" and the "scientific."²²

Apparatuses are not inscription devices, scientific instruments set in place before the action happens, or machines that mediate the dialectic of resistance and accommodation. They are neither neutral probes of the natural world nor structures that deterministically impose some particular outcome. In my further elaboration of Bohr's insights, apparatuses are not mere static arrangements *in* the world, but rather *apparatuses are dynamic (re)configurings of the world, specific agential practices/intra-actions/performances through which specific exclusionary boundaries are enacted*. Apparatuses have no inherent "outside" boundary. This indeterminacy of the "outside" boundary represents the impossibility of closure—the ongoing intra-activity in the iterative reconfiguring of the apparatus of bodily production. Apparatuses are open-ended practices.

Importantly, apparatuses are themselves phenomena. For example, as scientists are well aware, apparatuses are not preformed interchangeable objects that sit atop a shelf waiting to serve a particular purpose. Appa-

meaning to the notion of "wave," while the second provides determinate meaning to the notion of "particle." The notions of "wave" and "particle" do not refer to inherent characteristics of an object that precedes its intra-action. *There are no such independently existing objects with inherent characteristics*. The two different apparatuses effect different cuts, that is, draw different distinctions delineating the "measured object" from the "measuring instrument." In other words, they differ in their local material resolutions of the inherent ontological indeterminacy. There is no conflict because the two different results mark different intra-actions. See Barad 1996, forthcoming for more details.

²² This elaboration is not based on an analogical extrapolation. Rather, I argue that such anthropocentric restrictions to laboratory investigations are not justified and indeed defy the logic of Bohr's own insights. See Barad forthcoming.

ratuses are constituted through particular practices that are perpetually open to rearrangements, rearticulations, and other reworkings. This is part of the creativity and difficulty of doing science: getting the instrumentation to work in a particular way for a particular purpose (which is always open to the possibility of being changed during the experiment as different insights are gained). Furthermore, any particular apparatus is always in the process of intra-acting with other apparatuses, and the en-folding of locally stabilized phenomena (which may be traded across laboratories, cultures, or geopolitical spaces only to find themselves differently materializing) into subsequent iterations of particular practices constitutes important shifts in the particular apparatus in question and therefore in the nature of the intra-actions that result in the production of new phenomena, and so on. Boundaries do not sit still.

With this background we can now return to the question of the nature of phenomena. Phenomena are produced through agential intra-actions of multiple apparatuses of bodily production. Agential intra-actions are specific causal material enactments that may, or may not involve "humans." Indeed, it is through such practices that the differential boundaries between "humans" and "nonhumans," "culture" and "nature," the "social" and the "scientific" are constituted. Phenomena are constitutive of reality. Reality is not composed of things-in-themselves or things-behind-phenomena but "things"-in-phenomena.²⁵ The world *is* intra-activity in its differential mattering. It is through specific intra-actions that a differential sense of being is enacted in the ongoing ebb and flow of agency. That is, it is through specific intra-actions that phenomena come to matter—in both senses of the word. The world is a dynamic process of intra-activity in the ongoing reconfiguring of locally determinate causal structures with determinate boundaries, properties, meanings, and patterns of marks on bodies. This ongoing flow of agency through which "part" of the world makes itself differentially intelligible to another "part" of the world and through which local causal structures, boundaries, and properties are stabilized and destabilized does not take place in space and time but in the making of spacetime itself. The world is an ongoing open process of mattering through which "mattering" itself acquires meaning and form in the realization of different agential possibilities. Temporality and spatiality emerge in this processual his-

²⁵ Because phenomena constitute the ontological primitives, it makes no sense to talk about independently existing things as somehow behind or as the causes of phenomena. In essence, there are no noumena, only phenomena. Agential realist phenomena are neither Kant's phenomena nor the phenomenologist's phenomena.

toricity. Relations of exteriority, connectivity, and exclusion are reconfigured. The changing topologies of the world entail an ongoing reworking of the very nature of dynamics.

In summary, the universe is agential intra-activity in its becoming. The primary ontological units are not "things" but phenomena—dynamic topological reconfigurings/entanglements/relationalities/(re)articulations. And the primary semantic units are not "words" but material-discursive practices through which boundaries are constituted. This dynamism *is* agency. Agency is not an attribute but the ongoing reconfigurings of the world. On the basis of this performative metaphysics, in the next section I propose a posthumanist refiguration of the nature of materiality and discursivity and the relationship between them, and a posthumanist account of performativity.

A posthumanist account of material-discursive practices

Discursive practices are often confused with linguistic expression, and meaning is often thought to be a property of words. Hence, discursive practices and meanings are said to be peculiarly human phenomena. But if this were true, how would it be possible to take account of the boundary-making practices by which the differential constitution of "humans" and "nonhumans" are enacted? It would be one thing if the notion of constitution were to be understood in purely epistemic terms, but it is entirely unsatisfactory when questions of ontology are on the table. If "humans" refers to phenomena, not independent entities with inherent properties but rather beings in their differential becoming, particular material (re)configurings of the world with shifting boundaries and properties that stabilize and destabilize along with specific material changes in what it means to be human, then the notion of discursivity cannot be founded on an inherent distinction between humans and nonhumans. In this section, I propose a posthumanist account of discursive practices. I also outline a concordant reworking of the notion of materiality and hint at an agential realist approach to understanding the relationship between discursive practices and material phenomena.

Meaning is not a property of individual words or groups of words. Meaning is neither intralinguistically conferred nor extralinguistically referenced. Semantic contentfulness is not achieved through the thoughts or performances of individual agents but rather through particular discursive practices. With the inspiration of Bohr's insights, it would also be tempting to add the following agential realist points: meaning is not ide-

ational but rather specific material (re)configurings of the world, and semantic indeterminacy, like ontological indeterminacy, is only locally resolvable through specific intra-actions. But before proceeding, it is probably worth taking a moment to dispel some misconceptions about the nature of discursive practices.

Discourse is not a synonym for language.²⁴ Discourse does not refer to linguistic or signifying systems, grammars, speech acts, or conversations. To think of discourse as mere spoken or written words forming descriptive statements is to enact the mistake of representationalist thinking. Discourse is not what is said; it is that which constrains and enables what can be said. Discursive practices define what counts as meaningful statements. Statements are not the mere utterances of the originating consciousness of a unified subject; rather, statements and subjects emerge from a field of possibilities. This field of possibilities is not static or singular but rather is a dynamic and contingent multiplicity.

According to Foucault, discursive practices are the local sociohistorical material conditions that enable and constrain disciplinary knowledge practices such as speaking, writing, thinking, calculating, measuring, filtering, and concentrating. Discursive practices produce, rather than merely describe, the "subjects" and "objects" of knowledge practices. On Foucault's account these "conditions" are immanent and historical rather than transcendental or phenomenological. That is, they are not conditions in the sense of transcendental, ahistorical, cross-cultural, abstract laws defining the possibilities of experience (Kant), but rather they are actual historically situated social conditions.

Foucault's account of discursive practices has some provocative resonances (and some fruitful dissonances) with Bohr's account of apparatuses and the role they play in the material production of bodies and meanings. For Bohr, apparatuses are particular physical arrangements that give meaning to certain concepts to the exclusion of others; they are the local physical conditions that enable and constrain knowledge practices such as conceptualizing and measuring; they are productive of (and part of) the phenomena produced; they enact a local cut that produces "objects" of particular knowledge practices within the particular phenomena produced. On the basis of his profound insight that "concepts" (which are actual physical arrangements) and "things" do not have determinate boundaries,

²⁴ I am concerned here with the Foucauldian notion of discourse (discursive practices), not formalist and empirical approaches stemming from Anglo-American linguistics, sociolinguistics, and sociology.

properties, or meanings apart from their mutual intra-actions, Bohr offers a new epistemological framework that calls into question the dualisms of object/subject, knower/known, nature/culture, and word/world.

Bohr's insight that concepts are not ideational but rather are actual physical arrangements is clearly an insistence on the materiality of meaning making that goes beyond what is usually meant by the frequently heard contemporary refrain that writing and talking are material practices. Nor is Bohr merely claiming that discourse is "supported" or "sustained" by material practices, as Foucault seems to suggest (though the nature of this "support" is not specified), or that nondiscursive (background) practices determine discursive practices, as some existential-pragmatic philosophers purport.²⁶ Rather, Bohr's point entails a much more intimate relationship between concepts and materiality. In order to better understand the nature of this relationship, it is important to shift the focus from linguistic concepts to discursive practices.

On an agential realist elaboration of Bohr's theoretical framework, apparatuses are not static arrangements in the world that embody particular concepts to the exclusion of others; rather, apparatuses are specific material practices through which local semantic and ontological determinacy are intra-actively enacted. That is, apparatuses are the exclusionary practices of mattering through which intelligibility and materiality are constituted. Apparatuses are material (re)configurings/discursive practices that produce material phenomena in their discursively differentiated becoming. A phenomenon is a dynamic relationality that is locally determinate in its matter and meaning as mutually determined (within a particular phenomenon) through specific causal intra-actions. Outside of particular agential intra-actions, "words" and "things" are indeterminate. Hence, the notions of materiality and discursivity must be reworked in a way that acknowledges their mutual entailment. In particular, on an agential realist account, both materiality and discursive practices are rethought in terms of intra-activity.

On an agential realist account, *discursive practices are specific material*

²⁶ Foucault makes a distinction between "discursive" and "nondiscursive" practices, where the latter category is reduced to social institutional practices: "The term 'institution' is generally applied to every kind of more-or-less constrained behaviour, everything that functions in a society as a system of constraint and that isn't utterance, in short, *all the field of the non-discursive social, is an institution*" (1980b, 197–98; my italics). This specific social science demarcation is not particularly illuminating in the case of agential realism's posthumanist account, which is not limited to the realm of the social. In fact, it makes no sense to speak of the "nondiscursive" unless one is willing to jettison the notion of causality in its intra-active conception.

(re)configurings of the world through which local determinations of boundaries, properties, and meanings are differentially enacted. That is, discursive practices are ongoing agential intra-actions of the world through which local determinacy is enacted within the phenomena produced. Discursive practices are causal intra-actions—they enact local causal structures through which one “component” (the “effect”) of the phenomenon is marked by another “component” (the “cause”) in their differential articulation. Meaning is not a property of individual words or groups of words but an ongoing performance of the world in its differential intelligibility. In its causal intra-activity, “part” of the world becomes determinately bounded and propertied in its emergent intelligibility to another “part” of the world. Discursive practices are boundary-making practices that have no finality in the ongoing dynamics of agential intra-activity.

Discursive practices are not speech acts, linguistic representations, or even linguistic performances, bearing some unspecified relationship to material practices. Discursive practices are not anthropomorphic placeholders for the projected agency of individual subjects, culture, or language. Indeed, they are not human-based practices. On the contrary, agential realism’s posthumanist account of discursive practices does not fix the boundary between “human” and “nonhuman” before the analysis ever gets off the ground but rather enables (indeed demands) a genealogical analysis of the discursive emergence of the “human.” “Human bodies” and “human subjects” do not preexist as such; nor are they mere end products. “Humans” are neither pure cause nor pure effect but part of the world in its open-ended becoming.

Matter, like meaning, is not an individually articulated or static entity. Matter is not little bits of nature, or a blank slate, surface, or site passively awaiting signification; nor is it an uncontested ground for scientific, feminist, or Marxist theories. Matter is not a support, location, referent, or source of sustainability for discourse. Matter is not immutable or passive. It does not require the mark of an external force like culture or history to complete it. Matter is always already an ongoing historicity.²⁶

²⁶ In her critique of constructivism within feminist theory Judith Butler puts forward an account of materialization that seeks to acknowledge these important points. Reworking the notion of matter as a process of materialization brings to the fore the importance of recognizing matter in its historicity and directly challenges representationalism’s construal of matter as a passive blank site awaiting the active inscription of culture and the representationalist positioning of the relationship between materiality and discourse as one of absolute exteriority. Unfortunately, however, Butler’s theory ultimately reinscribes matter as a passive product of discursive practices rather than as an active agent participating in the very process of materialization. This deficiency is symptomatic of an incomplete assessment of important

On an agential realist account, matter does not refer to a fixed substance; rather, *matter is substance in its intra-active becoming—not a thing, but a doing, a congealing of agency. Matter is a stabilizing and destabilizing process of iterative intra-activity.* Phenomena—the smallest material units (relational “atoms”)—come to matter through this process of ongoing intra-activity. That is, *matter refers to the materiality/materialization of phenomena*, not to an inherent fixed property of abstract independently existing objects of Newtonian physics (the modernist realization of the Democritean dream of atoms and the void).

Matter is not simply “a kind of citationality” (Butler 1993, 15), the surface effect of human bodies, or the end product of linguistic or discursive acts. Material constraints and exclusions and the material dimensions of regulatory practices are important factors in the process of materialization. The dynamics of intra-activity entails matter as an *active* “agent” in its ongoing materialization.

Boundary-making practices, that is, discursive practices, are fully implicated in the dynamics of intra-activity through which phenomena come to matter. In other words, materiality is discursive (i.e., material phenomena are inseparable from the apparatuses of bodily production: matter emerges out of and includes as part of its being the ongoing reconfiguring of boundaries), just as discursive practices are always already material (i.e., they are ongoing material (re)configurings of the world). Discursive practices and material phenomena do not stand in a relationship of externality to one another; rather, the material and the discursive are mutually implicated in the dynamics of intra-activity. But nor are they reducible to one another. The relationship between the material and the discursive is one of mutual entailment. Neither is articulated/articulable in the absence of the other; matter and meaning are mutually articulated. Neither discursive practices nor material phenomena are ontologically or epistemologically prior. Neither can be explained in terms of the other. Neither has privileged status in determining the other.

Apparatuses of bodily production and the phenomena they produce are material-discursive in nature. *Material-discursive practices are specific iterative enactments—agential intra-actions—through which matter is dif-*

causal factors and an incomplete reworking of “causality” in understanding the nature of discursive practices (and material phenomena) in their productivity. Furthermore, Butler’s theory of materiality is limited to an account of the materialization of human bodies or, more accurately, to the construction of the contours of the human body. Agential realism’s relational ontology enables a further reworking of the notion of materialization that acknowledges the existence of important linkages between discursive practices and material phenomena without the anthropocentric limitations of Butler’s theory.

ferentially engaged and articulated (in the emergence of boundaries and meanings), reconfiguring the material-discursive field of possibilities in the iterative dynamics of intra-activity that is agency. Intra-actions are causally constraining nondeterministic enactments through which matter-in-the-process-of-becoming is sedimented out and enfolded in further materializations.²⁷

Material conditions matter, not because they "support" particular discourses that are the actual generative factors in the formation of bodies but rather because *matter comes to matter* through the iterative intra-activity of the world in its becoming. The point is not merely that there are important material factors in addition to discursive ones; rather, the issue is the conjoined material-discursive nature of constraints, conditions, and practices. The fact that material and discursive constraints and exclusions are intertwined points to the limited validity of analyses that attempt to determine individual effects of material or discursive factors.²⁸ Furthermore, the conceptualization of materiality offered by agential realism makes it possible to take account of material constraints and conditions once again without reinscribing traditional empiricist assumptions concerning the transparent or immediate given-ness of the world and without falling into the analytical stalemate that simply calls for a recognition of our mediated access to the world and then rests its case. The ubiquitous pronouncements proclaiming that experience or the material world is "mediated" have offered precious little guidance about how to proceed. The notion of mediation has for too long stood in the way of a more thoroughgoing accounting of the empirical world. The reconceptualization of materiality offered here makes it possible to take the empirical world seriously once again, but this time with the understanding that the objective referent is phenomena, not the seeming "immediately given-ness" of the world.

All bodies, not merely "human" bodies, come to matter through the world's iterative intra-activity—its performativity. This is true not only of the surface or contours of the body but also of the body in the fullness of its physicality, including the very "atoms" of its being. Bodies are not objects with inherent boundaries and properties; they are material-discursive phenomena. "Human" bodies are not inherently different from "nonhuman" ones. What constitutes the "human" (and the "nonhuman") is not a fixed or pregiven notion, but nor is it a free-floating ideality. What is at issue is not some ill-defined process

²⁷ The nature of causal intra-actions is discussed further in the next section.

²⁸ See Barad 1998b, 2001a, 2001b, forthcoming for examples.

by which human-based linguistic practices (materially supported in some unspecified way) manage to produce substantive bodies/bodily substances but rather a material dynamics of intra-activity: material apparatuses produce material phenomena through specific causal intra-actions, where “material” is always already material-discursive—*that is what it means to matter*. Theories that focus exclusively on the materialization of “human” bodies miss the crucial point that the very practices by which the differential boundaries of the “human” and the “nonhuman” are drawn are always already implicated in particular materializations. The differential constitution of the “human” (“non-human”) is always accompanied by particular exclusions and always open to contestation. This is a result of the nondeterministic causal nature of agential intra-actions, a crucial point that I take up in the next section.

The nature of production and the production of nature: Agency and causality

What is the nature of causality on this account? What possibilities exist for agency, for intervening in the world’s becoming? Where do the issues of responsibility and accountability enter in?

Agential intra-actions are causal enactments. Recall that an agential cut effects a local separability of different “component parts” of the phenomenon, one of which (“the cause”) expresses itself in effecting and marking the other (“the effect”). In a scientific context this process is known as a “measurement.” (Indeed, the notion of “measurement” is nothing more or less than a causal intra-action.)²⁹ Whether it is thought of as a “measurement,” or as part of the universe making itself intelligible to another part in its ongoing differentiating intelligibility and materialization, is a matter of preference.³⁰ Either way, what is important about causal intra-actions is the fact that marks are left on bodies. Objectivity means being accountable to marks on bodies.

This causal structure differs in important respects from the common choices of absolute exteriority and absolute interiority and of determinism

²⁹ I am grateful to Joe Rouse for putting this point so elegantly (private conversation). Rouse (2002) suggests that *measurement* need not be a term about laboratory operations, that before answering whether or not something is a measurement a prior question must be considered, namely, What constitutes a measurement of what?

³⁰ Intelligibility is not a human-based affair. It is a matter of differential articulations and differential responsiveness/engagement. Vicki Kirby (1997) makes a similar point.

and free will. In the case of the geometry of absolute exteriority, the claim that cultural practices produce material bodies starts with the metaphysical presumption of the ontological distinction of the former set from the latter. The inscription model of constructivism is of this kind: culture is figured as an external force acting on passive nature. There is an ambiguity in this model as to whether nature exists in any prediscursive form prior to its marking by culture. If there is such an antecedent entity then its very existence marks the inherent limit of constructivism. In this case, the rhetoric should be softened to more accurately reflect the fact that the force of culture "shapes" or "inscribes" nature but does not materially *produce* it. On the other hand, if there is no preexistent nature, then it behooves those who advocate such a theory to explain how it is that culture can materially produce that from which it is allegedly ontologically distinct, namely nature. What is the mechanism of this production? The other usual alternative is also not attractive: the geometry of absolute interiority amounts to a reduction of the effect to its cause, or in this case nature to culture, or matter to language, which amounts to one form or another of idealism.

Agential separability presents an alternative to these unsatisfactory options.³¹ It postulates a sense of "exteriority within," one that rejects the previous geometries and opens up a much larger space that is more appropriately thought of as a changing topology.³² More specifically, *agential separability* is a matter of *exteriority within* (*material-discursive*) *phenomena*. Hence, no priority is given to either materiality or discursivity.³³ There

³¹ Butler also rejects both of these options, proposing an alternative that she calls the "constitutive outside." The "constitutive outside" is an exteriority *within language*—it is the "that which" to which language is impelled to respond in the repeated attempt to capture the persistent loss or absence of that which cannot be captured. It is this persistent demand for, and inevitable failure of, language to resolve that demand that opens up a space for resignification—a form of agency—within the terms of that reiteration. But the fact that language itself is an enclosure that contains the constitutive outside amounts to an unfortunate reinscription of matter as subservient to the play of language and displays a commitment to an unacceptable anthropocentrism, reducing the possibilities for agency to resignification.

³² Geometry is concerned with shapes and sizes (this is true even of the non-Euclidean varieties, such as geometries built on curved surfaces like spheres rather than on flat planes), whereas topology investigates questions of connectivity and boundaries. Although spatiality is often thought of geometrically, particularly in terms of the characteristics of enclosures (like size and shape), this is only one way of thinking about space. Topological features of manifolds can be extremely important. For example, two points that seem far apart geometrically may, given a particular connectivity of the spatial manifold, actually be proximate to one another (as, e.g., in the case of cosmological objects called "wormholes").

³³ In contrast to Butler's "constitutive outside," for example.

is no geometrical relation of absolute exteriority between a “causal apparatus” and a “body effected,” nor an idealistic collapse of the two, but rather an ongoing topological dynamics that enfolds the spacetime manifold upon itself, a result of the fact that the apparatuses of bodily production (which are themselves phenomena) are (also) part of the phenomena they produce. Matter plays an active, indeed agential, role in its iterative materialization, but this is not the only reason that the space of agency is much larger than that postulated in many other critical social theories.³⁴ Intra-actions always entail particular exclusions, and exclusions foreclose any possibility of determinism, providing the condition of an open future.³⁵ Therefore, intra-actions are constraining but not determining. That is, intra-activity is neither a matter of strict determinism nor unconstrained freedom. The future is radically open at every turn. This open sense of futurity does not depend on the clash or collision of cultural demands; rather, it is inherent in the nature of intra-activity—even when apparatuses are primarily reinforcing, agency is not foreclosed. Hence, the notion of intra-actions reformulates the traditional notion of causality and opens up a space, indeed a relatively large space, for material-discursive forms of agency.

A posthumanist formulation of performativity makes evident the importance of taking account of “human,” “nonhuman,” and “cyborgian” forms of agency (indeed all such material-discursive forms). This is both possible and necessary because agency is a matter of changes in the apparatuses of bodily production, and such changes take place through various intra-actions, some of which remake the boundaries that delineate the differential constitution of the “human.” Holding the category “human” fixed excludes an entire range of possibilities in advance, eliding important dimensions of the workings of power.

On an agential realist account, agency is cut loose from its traditional humanist orbit. Agency is not aligned with human intentionality or subjectivity. Nor does it merely entail resignification or other specific kinds of moves within a social geometry of antihumanism. Agency is a matter of intra-acting; it is an enactment, not something that someone or some-

³⁴ For example, the space of agency is much larger than that postulated by Butler’s or Louis Althusser’s theories. There is more to agency than the possibilities of linguistic resignification, and the circumvention of deterministic outcome does not require a clash of apparatuses/discursive demands (i.e., overdetermination).

³⁵ This is true at the atomic level as well. Indeed, as Bohr emphasizes, the mutual exclusivity of “position” and “momentum” is what makes the notion of causality in quantum physics profoundly different from the determinist sense of causality of classical Newtonian physics.

thing has. Agency cannot be designated as an attribute of "subjects" or "objects" (as they do not preexist as such). Agency is not an attribute whatsoever—it is "doing"/"being" in its intra-activity. Agency is the enactment of iterative changes to particular practices through the dynamics of intra-activity. Agency is about the possibilities and accountability entailed in reconfiguring material-discursive apparatuses of bodily production, including the boundary articulations and exclusions that are marked by those practices in the enactment of a causal structure. Particular possibilities for acting exist at every moment, and these changing possibilities entail a responsibility to intervene in the world's becoming, to contest and rework what matters and what is excluded from mattering.

Conclusions

Feminist studies, queer studies, science studies, cultural studies, and critical social theory scholars are among those who struggle with the difficulty of coming to terms with the weightiness of the world. On the one hand, there is an expressed desire to recognize and reclaim matter and its kindred reviled Others exiled from the familiar and comforting domains of culture, mind, and history, not simply to altruistically advocate on behalf of the subaltern but in the hopes of finding a way to account for our own finitude. Can we identify the limits and constraints, if not the grounds, of discourse-knowledge in its productivity? But despite its substance, in the end, according to many contemporary attempts at its salvation, it is not matter that reels in the unruliness of infinite possibilities; rather, it is the very existence of finitude that gets defined as matter. Caught once again looking at mirrors, it is either the face of transcendence or our own image. It is as if there are no alternative ways to conceptualize matter: the only options seem to be the naïveté of empiricism or the same old narcissistic bedtime stories.

I have proposed a posthumanist materialist account of performativity that challenges the positioning of materiality as either a given or a mere effect of human agency. On an agential realist account, materiality is an active factor in processes of materialization. Nature is neither a passive surface awaiting the mark of culture nor the end product of cultural performances. The belief that nature is mute and immutable and that all prospects for significance and change reside in culture is a reinscription of the nature/culture dualism that feminists have actively contested. Nor, similarly, can a human/nonhuman distinction be hardwired into any theory that claims to take account of matter in the fullness of its historicity. Feminist science studies scholars in particular have emphasized that foundational inscriptions

of the nature/culture dualism foreclose the understanding of how "nature" and "culture" are formed, an understanding that is crucial to both feminist and scientific analyses. They have also emphasized that the notion of "formation" in no way denies the material reality of either "nature" or "culture." Hence, any performative account worth its salt would be ill advised to incorporate such anthropocentric values in its foundations.

A crucial part of the performative account that I have proposed is a rethinking of the notions of discursive practices and material phenomena and the relationship between them. On an agential realist account, discursive practices are not human-based activities but rather specific material (re)configurings of the world through which local determinations of boundaries, properties, and meanings are differentially enacted. And matter is not a fixed essence; rather, matter is substance in its intra-active becoming—not a thing but a doing, a congealing of agency. And performativity is not understood as iterative citationality (Butler) but rather iterative intra-activity.

On an agential realist account of technoscientific practices, the "knower" does not stand in a relation of absolute externality to the natural world being investigated—there is no such exterior observational point.³⁶ It is therefore not absolute exteriority that is the condition of possibility for objectivity but rather agential separability—exteriority within phenomena.³⁷ "We" are not outside observers of the world. Nor are we simply located at particular places *in* the world; rather, we are part *of* the world in its ongoing intra-activity. This is a point Niels Bohr tried to get at in his insistence that our epistemology must take account of the fact that we are a part of that nature we seek to understand. Unfortunately, however, he cuts short important posthumanist implications of this insight in his ultimately humanist understanding of the "we." Vicki Kirby eloquently articulates this important posthumanist point: "I'm trying to complicate the locatability of human identity as a here and now, an enclosed and finished product, a causal force upon Nature. Or even . . . as something within Nature. I don't want the human to be in Nature, as if Nature is a container. Identity is inherently unstable, differentiated, dispersed, and yet strangely coherent. If I say 'this is Nature itself,' an expression that

³⁶ Others have made this point as well, e.g., Haraway 1991; Kirby 1997; Rouse 2002; and Bohr.

³⁷ The notion of agential separability, which is predicated on the agential realist notion of intra-actions, has far-reaching consequences. Indeed, it can be shown to play a critical role in the resolution of the "measurement problem" and other long-standing problems in quantum theory. See Barad forthcoming.

usually denotes a prescriptive essentialism and that's why we avoid it, I've actually animated this 'itself' and even suggested that 'thinking' isn't the other of nature. Nature performs itself differently."²²

The particular configuration that an apparatus takes is not an arbitrary construction of "our" choosing; nor is it the result of causally deterministic power structures. "Humans" do not simply assemble different apparatuses for satisfying particular knowledge projects but are themselves specific local parts of the world's ongoing reconfiguring. To the degree that laboratory manipulations, observational interventions, concepts, or other human practices have a role to play it is as part of the material configuration of the world in its intra-active becoming. "Humans" are part of the world-body space in its dynamic structuration.

There is an important sense in which practices of knowing cannot be fully claimed as human practices, not simply because we use nonhuman elements in our practices but because knowing is a matter of part of the world making itself intelligible to another part. Practices of knowing and being are not isolatable, but rather they are mutually implicated. We do not obtain knowledge by standing outside of the world; we know because "we" are *of* the world. We are part of the world in its differential becoming. The separation of epistemology from ontology is a reverberation of a metaphysics that assumes an inherent difference between human and nonhuman, subject and object, mind and body, matter and discourse. *Onto-epistem-ology*—the study of practices of knowing in being—is probably a better way to think about the kind of understandings that are needed to come to terms with how specific intra-actions matter.

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²² Vicki Kirby (private communication, 2002). Kirby's sustained interrogation of the tenacious nature/culture binary is unparalleled. See Kirby 1997 for a remarkable "materialist" (my description) reading of Derridean theory.

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**Mannish Women, Passive Men, and Constitutional Types:
Margaret Mead's *Sex and Temperament in Three
Primitive Societies* as a Response to Ruth
Benedict's *Patterns of Culture***

The relationship between Ruth Benedict and Margaret Mead might be viewed as a conversation, one carried on in poems and letters, through phone calls and personal encounters, over many years. They met in 1922 in a class in anthropology at Barnard College. Mead, then twenty-one, was a student in the class, and Benedict, fifteen years older and a Ph.D. candidate in anthropology at Columbia, was the teaching assistant. Their friendship—as teacher and student, fictive mother and daughter, professional colleagues, and lovers—reached its initial intellectual culmination in two books, Benedict's *Patterns of Culture* (1934) and Mead's *Sex and Temperament in Three Primitive Societies* ([1935] 1963).¹

Published only a year apart, both books reached a large audience among academics and the general public. Foundational for anthropology, they were also reform texts that addressed a number of current issues. Each author focused on three tribal societies: Benedict on the Zunis of New Mexico, the Dobus of Melanesia, and the Kwakiutl of Vancouver Island, Canada; Mead on the Arapesh, the Mundugumor, and the Tchambuli—all in New Guinea. In both books, one society is peaceful (the Zunis for Benedict, the Arapesh for Mead), and the other two are more violent (the Dobus and the Kwakiutl for Benedict, the Mundugumor and the Tchambuli for Mead). Both authors identified the dominant “pattern” in the societies they examined, while they also pointed out social deviants to

¹ This article is based on my reading of the Ruth Benedict papers at Vassar College and the Margaret Mead papers at the Library of Congress, in addition to their published work and other material. Many personal letters in the Library of Congress, comprising hundreds of items, were opened to scholars in 2000 and 2001. No previous scholar has published from these formerly restricted papers; thus, my analysis revises previous biographical accounts of Benedict and Mead, including Modell 1983; Howard 1984; Caffrey 1989; and Lapeley 1999.

show that individuals classified as abnormal in one society might be considered normal in another. Both made a case for cultural relativism—that no society is innately superior to another—and both criticized the society of the United States. For her part, Mead took the conclusion about cultural relativism into the area of gender, challenging the standard belief that *masculine* and *feminine* are absolute categories defining male and female; indeed, she proposed that even *homosexuality* and *heterosexuality* are cultural constructions.²

Moreover, both books reflected their authors' personal situations and contain autobiographical elements. In *Sex and Temperament*, Mead referred only briefly—and with praise—to Benedict's *Patterns of Culture*. Yet she disputed a number of Benedict's conclusions without mentioning her mentor specifically by name. Mead questioned Benedict's brief for homosexuality, her definition of abnormality, her cultural determinism, and her romanticization of berdaches (cross-gendered men) in the societies of the Plains Indians.³ She thus took up issues relating to homosexuality, masculinity and femininity, and biology and race.

The purpose of this article is to probe Mead's response in *Sex and Temperament* to Benedict's *Patterns of Culture* in these areas, providing a new reading of these texts, especially of *Sex and Temperament*. I will also investigate the contradiction in Mead's thought on gender that some analysts have noted: she viewed the categories of *masculine* and *feminine* as cultural constructions, implying an equality between men and women, but at the same time she affirmed the categories of *male* and *female*, implying that procreation and motherhood place women in a separate category from men.⁴ I will further analyze Mead's ideas about *constitutional types*, a theory about the construction of individuals and societies

² On the dominance of the male-female binary in the 1930s, see Minton 1986; Meyrowitz 2002. Given Mead's definition of sexual identity and desire as ambiguous, she might be classified among Joseph Boone's "queer modernists," those writers in the 1920s and 1930s who viewed such categories as multifaceted and shifting and who foresaw the "queer" politics and analysis of today. Among his queer modernists, Boone includes Bruce Nugent ("Smoke, Lilies, and Jade" [1926]), Blair Niles (*Strange Brother* [1931]), Charles Henn Ford and Parker Tyler (*The Young and the Evil* [1933]), and Djuna Barnes (*Nightwood* [1936]). See Boone 1998, 204–87.

³ Recent writers on institutionalized homosexuality among Native Americans dislike the term *berdache*, given its derivation from the Arabic word for a boy prostitute. I follow Benedict and Mead in using the term, but I also use man-woman, following their alternative practice. In her definitive study, Sabine Lang (1998) suggests placing the preferred gender of the cross-gendered individual first (i.e., woman-man), although Benedict and Mead do the opposite.

⁴ See, e.g., Sanday 1980, 340–48.

that informed her conclusions in *Sex and Temperament*, taking them well beyond Benedict's relativist position in *Patterns of Culture*. Before I address these matters, however, it is necessary to establish the biographical context in which *Sex and Temperament* is set because Mead's relationship with Benedict—as well as her relationships with her second and third husbands, anthropologists Reo Fortune and Gregory Bateson—had an important influence in shaping the book. Indeed, as Mead approached the end of her research in New Guinea on which *Sex and Temperament* is based, she wrote to Benedict that her work had turned into a combination of fieldwork and “applied biography.”⁶ For Fortune and Bateson were also in New Guinea, and in the winter and spring of 1933 they engaged with Mead in discussions about the societies they were studying there in terms of their own personalities, in accord with Benedict's description of cultures as “personality writ large” (Mead 1974, 43). Benedict was not in New Guinea, but she was a silent partner in their interactions, as they centered their discussions around a draft of *Patterns of Culture* that Benedict had sent Mead the previous fall.

When exploring the biographies of influential thinkers such as Benedict and Mead, one can come to understand connections between ideas and even whole fields of thought (including scientific thought) that otherwise remain obscure. In this case, we will see, on the one hand, how Mead's happening to fall in love with a scientist (her third husband, Bateson) and, on the other, her assessment of the uses that the Nazis were making by the early 1930s of the kind of scientific theory that interested her shaped how she developed her account, not only of three primitive societies but also of society in general.

Biography

Benedict and Mead had become lovers in 1924, while remaining devoted to their respective husbands—Benedict to Stanley Benedict, a research chemist, and Mead to Luther Cressman, a part-time Episcopal priest and Ph.D. candidate in sociology at Columbia. That arrangement was in line with Benedict's belief that humans possess both a “heterosexual” and a “homosexual” side, with each side running on a different “wheel” and

⁶ Margaret Mead to Ruth Benedict, March 29, 1933, Margaret Mead Papers, Library of Congress, Washington, D.C. (hereafter LC), R-7.

with each needing a sexual partner of the same orientation to be fulfilled.⁶ In 1928 Mead divorced Cressman, married Fortune, and went off to do fieldwork in New Guinea with Fortune; by then the sexual ardor between Benedict and Mead had cooled. When Mead returned to New York City with Fortune a year later, Benedict cut off their sexual involvement because she did not want to compete with Fortune for Mead's affections.⁷ In the early 1930s, as Mead went to New Guinea on a second trip with Fortune, Benedict chose lesbianism as her sexual identity, separating from her husband to find a partner in another young woman, Natalie Raymond. Enthusiastic about her decision to give up intimate relationships with men, Benedict even speculated that her lesbian orientation might be innate (Mead 1977a, 176, and 1977c).

That possibility alarmed Mead, who was ambivalent about homosexuality as the sole identity for anyone, especially for her closest friend. From her 1928 *Coming of Age in Samoa* on, Mead praised homosexual behavior—as distinct from identity. She sometimes called it *foreplay*, adopting the term that appeared during the “sexual revolution” of the 1920s to describe the new and varied heterosexual sexual techniques that recognized the importance of the clitoris to female sexual response while continuing to emphasize male penetration.⁸ Mead thought that those new heterosexual techniques had been borrowed from homosexual practice (Mead 1928, 148, 150, 223, and 1936, 23). However, her enthusiasm for homosexual behavior did not extend to homosexuality as personal identity; in fact, she often viewed both female and male homosexuality in terms of a masculinity she did not like. Mead declared: “I don’t want to be a man.” And “the thought of possibly having one of those obscure masculinizing diseases fills me with more horror than any other maligned fate.”⁹ She stated that she often played the part of a mother or a daughter

⁶ Ruth Benedict to Margaret Mead, September 10, 1928, LC, S-4. I follow present-day usage with regard to words such as *bisexuality*, *homosexuality*, and *lesbianism*, although I am aware of their differing meanings over time. In fact, in the hundreds of letters between them, neither Benedict nor Mead used the terms *bisexual* or *lesbian* to refer to herself or the other, although Mead used the term *lesbian* to refer to other women that they knew. For *lesbian* they used the term *female homosexual*, and for *bisexual* they used the term *mixed type*. Mead first used the term *bisexual* in 1939, in the brief that she wrote for Walter Spies, on trial in Bali for sodomy with Balinese boys.

⁷ Ruth Benedict to Margaret Mead, March 29, 1933, LC, R-7.

⁸ On the new sex behavior in the 1920s, see Seidman 1991; Laipson 1996.

⁹ Houston interview with Mead, 323. Blackberry Winter (BW) draft chapter, “On Having a Baby,” 1. Jean Houston interviewed Mead shortly before her death, and the transcript of that interview (located in LC) is nearly five hundred pages long. The BW drafts are in LC, Publications File, I-204-8.

in her intimate relationships, but she never played a masculine role.¹⁰ She dressed in feminine clothing, wearing full skirts and white gloves—as all proper ladies did on formal occasions before the 1960s (Bateson 1984, 96). In 1934, Mead wrote to Bateson that she suspected that her sister Priscilla suspected that she, Margaret, was a lesbian but that she was not.¹¹ In 1938, using a coded phrase for her new sexual orientation, Benedict declared to Mead that she was a “denizen” with her in the “undiscovered country” that she had found. Mead disagreed. She replied that she might have helped Benedict to find that country but that she was not “a proper denizen.”¹²

In the late nineteenth century, sexologists such as Havelock Ellis proposed that lesbians possessed a male “soul” or “self” in a female body—with male homosexuals having a female soul in a male body. All homosexuality, they maintained, involved an “inversion” to the behavior and appearance of the other sex. Such ideas partly derived from their adherence to the binary division between male and female that was a core belief of Western culture—as well as to that culture’s inability to visualize the sex act without the male phallus. By the early 1900s, they had modified their ideas somewhat to admit that male homosexuals might be masculine. Women who desired only women, however, had to be male in orientation. That belief was in line with their view of sex as phallic and the dislike many felt for militant women in the suffrage and temperance movements—and sometimes even for career women in general. Thus, the sexologists constructed the “mannish” lesbian, whom they described as looking and dressing like a man.¹³

Mead wrote in *Sex and Temperament* that before she did her New Guinea research she accepted these ideas. “I shared the general belief of our society that there was a natural sex-temperament which could at the most only be distorted or diverted from normal expression.” She considered even what she called “mixed types”—individuals who were attracted to both sexes—to be composed of men with predominantly feminine

¹⁰ Margaret Mead to Ruth Benedict, January 7, 1939, LC, B-1; Margaret Mead to Marie Eichelberger, February 5, 1939, LC, B-4.

¹¹ Margaret Mead to Gregory Bateson, June 22, 1934, LC, R-1.

¹² Ruth Benedict to Margaret Mead, August 3, 1938, LC, S-5; Margaret Mead to Ruth Benedict, October 6, 14, 1938, LC, R-7. The word *denizen* means inhabitant, especially in the sense of an alien admitted to a new country.

¹³ The literature on the early sexologists is vast. In addition to reading the major contemporary works on the subject by Ellis, Sigmund Freud, Magnus Hirschfeld, and Edward Carpenter, I have also relied on Chauncey 1989, 87–117; Duggan 1993; Rosano 1997; Terry 1999; Meyerowitz 2002.

temperaments and women with predominantly masculine ones ([1935] 1963, xiv). As late as 1949, after years of thinking about these matters, Mead still had difficulty accepting that lesbian women were not "mannish" by nature. In *Male and Female*, published that year, she wrote, "We are continually confronted with what looks like a correlation between a tendency toward sex inversion of those who look like the opposite sex" (1949, 131).

She concluded that gender and sexual identity were fluid after finding that both genders were maternal among the Arapesh and aggressive among the Mundugumor, while the women of the Tchambuli were masculine and the men feminine—reversing the standard gender behaviors of Western cultures. If that were the case, the masculine-feminine binary of the West was not universal. The differing personalities of Fortune and Bateson were also important in her discovery that gender was not absolute. Fortune, a man of provincial background from New Zealand, was strongly heterosexual, with a drive to dominate women and a tendency toward jealousy; his aggressive masculinity resulted in several instances of physical aggression on his part toward her.¹⁴ "He came from a culture where boys were physically disciplined and men struck women," Mead wrote.¹⁵ Tired of Cressman's passivity and his somewhat plodding intellect, Mead was attracted to Fortune's masculine virility, his mental brilliance, and his good looks as well as to his poetic, sensitive side, but she did not entirely realize what she was getting into when she married him. For his part, Fortune had problems with Mead's sometimes domineering behavior—and with her fame from *Coming of Age in Samoa*.

Bateson was different. The scion of a family connected to the English intellectual aristocracy, educated at the elite Charterhouse School and Cambridge University, he had a gentle personality, a fluid sexual identity, and a history of involvements with both men and women. Like Mead, he was a believer in free love—the contemporary doctrine that sex was a force

¹⁴ Mead's papers contain references to two such episodes: one in Tchambuli in the late spring of 1933 and one in Sydney, Australia, that summer. Compare Houston's interview with Mead, 347; Margaret Mead to Gregory Bateson, May 30, 1934, LC, R-1; "Notes to GCB (Gottard Booth)," February 4, 1949, LC, R-10. Booth, a Jungian analyst, was Mead's therapist.

¹⁵ BW draft, "Sex and Temperament" chap. 6. When the gentle Arapesh frustrated him, according to Mead, he threatened them physically, and she had to intervene between them and his "lifted hand." "Threats of assault and battery, challenges to duels that were however never fought, was part of Reo's approach to the world" (BW draft, "Sex and Temperament," 6, 15).

flowing through humans and the universe that functioned best when individuals were both married and had lovers; Ellis had authored the major treatise on it.¹⁶ Bateson was more feminine than masculine, more like Mead than Fortune, they agreed, and his personality ratified her suspicions about the shifting nature of gender—and confirmed her growing belief that “passive males” were preferable to aggressive ones—even if, as was the case with Bateson, potency was a problem.¹⁷

Mead fell deeply in love with Bateson. The minute that she met him, she wrote Benedict, her marriage to Fortune fell down “like a house of cards.”¹⁸ Bateson had been studying the cultures of the middle Sepik River for several years; he helped her to understand the Tchambuli and to realize that masculinity and femininity were cultural constructions. She also concluded, based on their temperaments and sexual orientations and on her failure to observe any individuals with a homosexual identity during this trip to New Guinea, that heterosexuality and homosexuality were also cultural constructions. The real variable was in temperamental qualities such as aggression and gentleness translated into sexual behavior.

By the spring of 1933, on the basis of her work in New Guinea, Mead wrote to Benedict that Benedict was not lesbian at all; even her definition of homosexuality and heterosexuality as two tracks within the self was not correct. “The kind of feeling which you classified as ‘homosexual’ and ‘heterosexual,’” she wrote, “is really sex adapted to like or understood temperaments versus sex adapted to a relationship of strangeness and difference. I believe every person of ordinary sex endowment has a capacity of diffuse ‘homosexual’ sex expression and specific climax—according to the temperamental situation. To call men who prefer the diffuse experience ‘feminine’ or women who feel only the specific ‘masculine’ or both ‘mixed types’ is a lot of obfuscation—or nonsense.”¹⁹ By Mead’s calculus, Benedict was a “woman”; whatever her behavior or preference in the sex act, she was not “masculine” or even a “mixed type.” She informed Benedict:

¹⁶ The major texts on free love were Ellis (1906) 1936, 505–75; and Carpenter 1908. Mead described her free-love beliefs in a letter to her sister Elizabeth from Samoa (Margaret Mead to Elizabeth Mead, January 10, 1926, LC, Q-10.) She practiced them during her marriages to Cressman and Bateson, although not during her marriage to Fortune. Benedict and Mead resumed their sexual relationship during her marriage to Bateson.

¹⁷ Margaret Mead to Erik Erikson, April 27, 1947, LC, R-8; Margaret Mead to Elizabeth Hellensberg, August 4, 1947, LC, R-10; Margaret Mead to Gregory Bateson, November 27, 1948, LC, R-3.

¹⁸ Margaret Mead to Ruth Benedict, March 29, 1933, LC, R-7.

¹⁹ Ibid.

"You're a perfectly good woman, who prefers different temperamental types and responds differently to them."²⁰ The implication was that Benedict had a range of sexual responses, depending on her sexual partner, and that, given the right sort of temperament, she would enjoy a man as much as a woman.

Homosexuality

In *Patterns of Culture*, Benedict defended homosexuality, which she seemed to define as an identity, referring to the homosexual as an "invert." And she contended that whatever neuroses such individuals displayed in the United States resulted from the way that society treated them, not from any innate predisposition toward abnormality on their part. Furthermore, she maintained that some societies honored homosexuality, as did ancient Greece and certain Native American societies, especially those of the Plains. The latter did so, Benedict asserted, because their men-women crossed over gender boundaries to perform work not only done by men but also that done by women. Thus, they had especially prosperous—and envied—households ([1935] 1959, 262, 264–65).²¹

Mead disagreed. She viewed the adult males of the Plains Indians as insisting on aggressive masculinity so "frantically" that they drove gentle males into becoming berdaches. She further contended that the regular members of their societies laughed at these men-women ([1935] 1963, xii). Indeed, in *Sex and Temperament*, Mead looked favorably on societies without homosexuality. She described the Arapesh as being without any "psychosexual maladjustment"—by which she meant homosexuality as identity. When the gentle Arapesh men left their culture to venture into the outside world, they were easily raped or seduced into homosexual behavior because they did not know how to resist, but she did not observe any homosexuality among them in their own cultural setting. In fact, she contended that the Arapesh males were so gentle that there was no one among them to take on the active role in active-passive homosexual re-

²⁰ Ibid.

²¹ Benedict derived her information on the Plains berdaches from an unpublished manuscript by Ella Deloria, a Dakota Native American and a Ph.D. candidate in anthropology at Columbia University. In 1935, she stated that she feared Deloria had romanticized her subject (Ruth Benedict to Robert H. Lowie, April 17, 1935, Ruth Benedict Papers, Vassar College, Poughkeepsie, N.Y.). By 1939 Benedict had revised her conclusions, noting that the Dakota men-women did not engage in the active role in the sex act, while they regarded the berdache with ambivalence (Benedict 1930, 72).

lationshiPs, which she defined elsewhere as the dominant form of European homosexuality.²²

Yet her attitude toward homosexuality was not simple. In this case her conclusions were related to her views about masculinity. For it was not all homosexuals that she disliked, only the aggressive, active male in active-passive sex. And she did not discuss lesbianism in *Sex and Temperament* aside from Tchambuli women who engaged in "rough" homosexual behavior in dances—making them sound like stereotypical "mannish lesbians." Mead later maintained that women were "cherishing" when they engaged in sexual relations with each other, but homosexual men emphasized "emotional asymmetry" when they had sex with each other.²³

Moreover, she not only criticized homosexual men for domineering behavior but also criticized heterosexual males for the same behavior. Present-day men's studies theorists have called that sort of masculine behavior "hegemonic" to distinguish it from other sorts.²⁴ After several years of marriage to Fortune, Mead especially disliked aggressive men. In her friend-of-the-court brief for Walter Spies, which she wrote on Bali in 1939, Mead contended that active homosexual males often began as heterosexual, but their drive for power was so strong that dominating women eventually did not satisfy them and so they turned to passive males and boys. Passive males, on the other hand, took up that position because their mothers were domineering or because they spent time in single-sex male institutions. In 1962, she criticized men in general for "phallic athleticism" (Mead 1962, 11). Moreover, Mead did not like the masculine aggressiveness of the Plains Indians—nor that of both the men and the women of the Mundugumor people. She also disliked the aggressive behavior of a group of young males in Tchambuli, who were by her description "violent, possessive, and actively sexed, intolerant of any control." She called them "viriloid" and identified them as the central "deviants" in Tchambuli society ([1935] 1963, 271).

Mead called the "viriloid" Tchambuli youth "actively sexed"; she called the masculinized Mundugumor people "positively sexed" ([1935] 1963, 279). By "actively sexed" or "positively sexed" men, she meant males who practiced the missionary position and often were insensitive to their

²² Margaret Mead, "Background Statement about Homosexuality" (Spies brief), 1939, LC, N-30.

²³ Margaret Mead, draft essay on lesbianism for *Redbook*, February 15, 1971, LC, I-98. She was also critical of what she saw as a male homosexual tendency toward promiscuity and prostitution (Margaret Mead to Ruth Benedict, January 29, 1939, LC, B-1).

²⁴ On "hegemonic masculinity," see Brod and Kaufman 1994.

partners' needs.²⁵ By contrast, in *Sex and Temperament* she praised the gentle behavior of the Arapesh men in sex who, she implied, were as interested in loving behavior as in ejaculation. They gave the lie, she contended, to the Western belief that men were "spontaneously sexual" ([1935] 1963, 101). "Society," she later wrote, "cannot exist where there is too much aggression among the males."²⁶ By the time she left New Guinea, furious at Fortune's aggressive behavior toward her, she had decided that the best kind of marriage is between gentle individuals, both of whom are passive in sex. That conclusion justified her marriage to the "passive" Bateson.

In *Patterns of Culture*, Benedict identified homosexuals as a distinct group, one with a distinct identity. Reacting to Benedict's lesbianism, Mead challenged that stance by borrowing Sigmund Freud's idea that everyone is born with a diffuse sexual drive and that, even though mature individuals proceed through the Oedipus complex to become heterosexual, a "latent homosexuality" exists in them. The existence of "latent homosexuality" had not especially bothered Freud, but by the 1930s conservative Freudians maintained that the condition posed a major threat to individuals and to society. Wilhelm Stekel did so in the excerpts from his book *Bi-Sexual Love* (1933) that were included in the packet of background materials for the 1934 interdisciplinary conference that Mead attended, composed of sociologists, psychologists, and a biologist, for designing a high school curriculum on human behavior.²⁷ In *Sex and Temperament*, Mead turned Stekel's argument on its head. She contended that "latent homosexuality" was not the problem; rather, individuals suffered because they had to repress that side of themselves—and society suffered because the repression was so widespread. In *Patterns of Culture*, Benedict had confined her "abnormals" to homosexuals, the mentally disturbed, and male patriarchs in U.S. society. In a near Swiftian reversal of reality, Mead argued that almost everyone in American society was bisexual. Because they did not give in to their homosexual side, they were highly neurotic—and culturally deviant ([1935] 1963, 296).

According to Mead, in the United States everyone underwent a draconian socialization to stereotypical gender roles in childhood. "Ways of sitting or relaxing, ideas of sportsmanship and fair play, patterns of ex-

²⁵ Margaret Mead, "Summary Statement of the Problem of Personality and Culture," Tchambuli Field Notes, LC, N-102.

²⁶ Mead, "Background Statement about Homosexuality, 1939," 8.

²⁷ The conference was funded by the Rockefeller Foundation and held at the Hanover Inn in New Hampshire. See LC, F-31, F-32.

pressing emotions, and a multitude of other points"—all were imposed on children to force them to identify themselves as masculine or feminine. As a result, they became extremely neurotic. "Every time the point of sex-conformity is made, every time the child's sex is invoked as the reason why it should prefer trousers to petticoats, there is planted in the child's mind the fear it may not really belong to its own sex at all" ([1935] 1963, 297).

Moreover, the adherence to the masculine-feminine binary meant that individuals were unfortunately raised to repress their homosexual side as adults. The woman who became "mannish" might not "cross over sex lines" unless society defined what was often mildly masculine behavior on her part as inappropriate for her sex, thus forcing her to decide that she was a man. And such women often considered themselves unsuited for childbearing. With regard to men, raised to be domineering, as adults they often turned to submissive men as sex partners in single-sex groups. They thereby increased their anxiety about their masculinity, since a true man was supposed to desire only women. Or confronting a dominant woman, they might doubt their masculinity. And the men who conformed most closely to the dominant role for males were "the most suspicious and hostile toward deviating women," while those individuals who did not follow the norm confused those who did. In everyone "a seed of doubt, of anxiety, is planted" about their sexual identity ([1935] 1963, 306).

Moreover, Mead believed that drawing rigid sex lines between masculine and feminine produced the Oedipus complex. Without the distinction in gender roles, children would not view their parents as entirely separate beings, nor identify with one over the other. And since they no longer identified with the same-sex parent, they would not turn to homosexuality ([1935] 1963, 301).

Mead saw a basic problem with homosexuality: such individuals in her view invariably decided not to have children, and Mead regarded procreation as central to individual and social well-being. Having a child, she told Bateson, was the final and irrevocable proof to her that she was female.²⁸ In *Sex and Temperament*, she wrote that in a society like the Arapesh, which did not have a masculine-feminine binary but retained the idea of males and females as different, there was much less "psychosexual" maladjustment than elsewhere because "one basic aspect of a child's position in the universe is left unchallenged: the genuineness of its membership in its own sex. It can continue to watch the mating behavior of

²⁸ Gregory Bateson to Margaret Mead, October 21, 1948, LC, R-3

its elders and pattern its hopes and expectations on it" ([1935] 1963, 296).

Mead hinted at her fears of her own homosexual drive when she wrote that if men and women lost their reproductive identity they lost their psychosexual security. She used metaphors of citizenship to describe the security of belonging to a reproductive group. In *Sex and Temperament*, she referred to the "inalienable rights" of being male or female; she criticized women who identified themselves as men for engaging in an act of "sex-disenfranchisement" ([1935] 1963, 298). In an article she published in 1935 in *Forum* magazine (then a public interest journal), she wrote: "A person without full sex membership is worse off than a man without a country" (1935, 301-3). Such statements reflected her disagreement with Benedict about her being a "denizen" in Benedict's new country.

From her observations of her three societies in New Guinea, Mead concluded that in societies like the Arapesh and Mundugumor, which did not have a masculine-feminine division in gender roles, homosexuality did not exist. "Without any contrast between the sexes and without any tradition of transvestitism [cross-dressing], a variation in temperamental preference does not result in either homosexuality or transvestitism" ([1935] 1963, 294). In other words, in societies in which both women and men were either mostly gentle or mostly aggressive, without a masculine-feminine divide, there would not be any homosexuality—except for the occasional "congenital invert."²⁹

Mead did not mean that homosexual behavior did not occur; it was homosexuality as an identity that was absent. Among the Tchambuli and Bateson's Iatmul, she found cross-gender behavior and frequent wordplay about anal intercourse. Yet the cultural prohibitions against homosexuality were so strong that few people had the nerve to take it as a precise identity. Even the feminized men whom she observed in Tchambuli were fully heterosexual.³⁰ Yet in *Sex and Temperament*, she did not make clear her real conclusion: that bisexuality was the healthiest sort of sexual identity so long as one remained connected to one's procreative group and was not aggressive in the sex act.

²⁹ Mead clarified this position in *Males and Females* (1962, 197): "With the exception of the occasional anatomically confused hermaphrodite, homosexuality is a combination of adult expectations and fears and a possibility latent in many children that will not be fully expressed if it is not given social recognition."

³⁰ In small tribal groups, she later proposed, "cultural loss in the absence of any individuals to fill the role may be rapid," although knowledge of the role might continue to exist in language and rituals (Mead 1961, 1452).

Men

In *Patterns of Culture*, Benedict was critical of domineering male behavior. She did not like such behavior among the Dobus and the Kwakiutl, and she especially did not like it in the United States, where she identified "family men," "officers of the law," and "businessmen" as "arrogant and unbridled egoists" ([1934] 1959, 277). In *Sex and Temperament*, Mead also criticized domineering men, but she advanced another line of reasoning with regard to men in contemporary society, one derived from her analysis of the Tchambuli. Mead described the adult men in that society, who wore elaborate codpieces and headdresses and walked with mincing steps, as the most neurotic men she had ever seen. The masculinized Tchambuli women, with "shaven, unadorned heads," looking "solid, preoccupied, and powerful," dominant and demanding in sex, had taken over the male role of economic provider, leaving the men with little to do except make masks and engage in rituals ([1935] 1963, 257).

In this second line of reasoning about men in *Sex and Temperament*, Mead viewed them as more maimed than domineering, more insecure than uncaring. In *Patterns of Culture*, Benedict had noted that all societies consider men's occupations more important than women's. Mead turned this observation on its head by concluding that men's occupations are considered superior because men's egos are fragile. In *Male and Female*, written more than a decade later, she carried the male insecurity thesis even further. In that work she contended that men envied women's anatomy and their childbearing role so much that they suffered much more from "womb envy" than women suffered from "penis envy" (1949, 78–104).

In Mead's own life, Bateson had problems with his masculine identity, and it was not hard to figure out that under his masculine bluster Fortune was insecure. While writing *Sex and Temperament*, Mead became involved with John Dollard, another aggressive man with a deeply sensitive side. She was beginning to form professional and personal friendships with men who would defend her against the male anthropologists who were already attacking her work as inaccurate and hastily done—long before Derek Freeman in 1983 launched his famous attack on her Samoan research on those grounds.³¹ Her new defenders included Erik Erikson and Erich Fromm, in addition to Dollard. She wanted to honor those men, who were mainly sociologists, psychiatrists, and psychoanalysts.³²

Mead's relationship with Bateson and other men in her life influenced

³¹ Edward Sapir, whom she had rejected as a lover, began the attack on her in 1929 (Sapir 1929, 519–34).

³² Margaret Mead to John Dollard, September 19, 1938, LC, N-10.

her argument about male insecurity in *Sex and Temperament*. She realized Bateson's sense of inadequate masculinity—and in real life she used her femininity to bolster him. Indeed, in *Coming of Age in Samoa*, she had provided support for the very masculine Fortune during her early years of involvement with him; in that book she viewed “homosexual sex” (translated into “foreplay” in heterosexual terms) as operating to improve “heterosexual sex” (Mead 1928, 148, 151–53). That perspective probably helped Fortune tolerate—inasmuch as he could—her relationship with Benedict.

The situation of men in the United States in the 1930s reinforced Mead's belief that masculine aggression resulted from male insecurity. Studies of men out of work during the depression showed that they blamed their predicament on themselves much more than on the economic system.²² The ideal of aggressive masculinity that had been dominant in the pre-World War I era of Western imperialism resurfaced in the 1930s, motivated by “a crisis in masculinity” among men, who felt impotent in the face of unemployment. Edmund Wilson and other literary critics called for a new masculine realism; it appeared, for example, in the popular “tough guy” of the “hard-boiled” detective fiction written by Dashiell Hammett and Raymond Chandler and in the figures of the gangster and the cowboy in films. Superheroes such as Dick Tracy and Superman were created, and Superman's secret identity was the bumbling Clark Kent. Dagwood, of Blondie and Dagwood, initially was meant to satirize the upper-class homosexual type before he became an effeminate man who relied on his domineering wife. He was, as Mead put it in *Sex and Temperament* ([1935] 1963, x), a “hen-pecked” husband or, as she put it in *Male and Female*, a Caspar Milquetoast type (Mead 1949, 135; Melosh 1993, 155–81; Breu 2000).

Biology

In line with her cultural determinism, Benedict rejected the influence of biology as a significant factor shaping individuals or cultures. Despite assertions by biologists, Benedict wrote, they had not proved the influence on human development of heredity or physiology in terms of “basal metabolism” or “the functioning of the ductless glands” ([1934] 1959, 234). That stance created a problem for Mead, who had thought at the beginning of her research on New Guinea that she should investigate biological influences but had given up the idea because neither she nor Fortune

²² For studies of unemployed men in the 1930s, see Banner 1995, 214.

knew any biology. Then she met Bateson, and that changed everything. The son of William Bateson, one of the greatest geneticists of the day, Gregory Bateson himself was a trained biologist. He thought like a biologist, Mead wrote, "moving easily from one science to another, making analogies, now from physics, now from geology."²⁴ Moreover, Mead was friendly with Earl Engle, an endocrinologist at Columbia University Medical School. In a letter to Bateson, she reported that Engle had told her about new research in his field that discovered the "sex" hormones to be so mixed up in men and women that he did not think that biologists could any longer specify what constituted a "man" or a "woman."²⁵

When she wrote *Sex and Temperament*, Mead wanted to please Benedict, who had been angry with her for leaving Fortune at first but who was now helping her to keep him in the dark about her continuing relationship with Bateson. Using Benedict's configurationist approach was her greatest tribute, for she might have employed other methodologies. In studying the Manus from the late fall of 1928 through the summer of 1929, she had used Alfred Radcliffe-Brown's functionalism, emphasizing social institutions and their interactions; in studying the Omaha Indians during the summer of 1930, she had used Clark Wissler's assimilationism, emphasizing the impact of Westernization on a tribal society. Yet she also wanted to please Bateson, not only because she loved him but also because he knew a lot about the Tchambuli. Fortune, on the other hand, was furious with her for leaving him, and he refused to share his material on the Mundugumor and the Tchambuli with her, contending that the Arapesh were violent, not maternal, that they believed in a difference between masculine and feminine, and that she was wrong about them.²⁶

Needing Benedict's and Bateson's support and intellectually drawn to the configurationist approach and biological reasoning, Mead tried to

²⁴ Mead, BW draft, "Sex and Temperament," 13-14.

²⁵ Margaret Mead to Gregory Bateson, December 14, 1934, LC, R-2.

²⁶ On February 7, 1934, Mead wrote to Bateson that if they had not had the discussions in Tchambuli, she would feel "pretty sad about the Tchambuli work; I certainly did precious little" (LC, R-2). She spent eight months among the Arapesh, later writing a multivolume ethnography on them. She spent three months among the Mudugumor and four among the Tchambuli. In *Sex and Temperament*, she devoted a much larger space to the Arapesh than to the other two societies; she justified this by stating that Fortune had been responsible for much of the ethnography of the Mundugumor and the Tchambuli ([1935] 1963, i). Some of the confusion in this work stemmed from various individuals having input into its writing, including her editor at William Morrow. She wrote to Bateson, February 21, 1935: "I have decided that I had better start to learn to write and stop depending on others to pick up the pieces and tie up the loose ends" (LC, S-4). Unfortunately, Mead's papers include no drafts of *Sex and Temperament*.

include both. She titled her book "Sex and Temperament," implying by the use of the term *temperament* that the book contained a good deal of biology. "Temperament," Mead wrote, "meant innate individual endowments" or, as she later defined it, "genetic makeup."³⁷ In the preface to *Sex and Temperament*, Mead thanked Engle for his help, but she did not include his information about hormonal research in the book. She mentioned birth order, twins, and infants born with a caul (a fetal membrane covering the head of a child at birth) as biological variations that could influence a culture's formation of its gender pattern, but genes and hormones are conspicuously absent. Moreover, Mead is confusing to a present-day reader because she did not use the word *gender* to indicate the cultural self as opposed to the biological self. That would not happen until feminists in the 1970s adopted the distinction between *sex* as biology and *gender* as culture from *sex* researchers at The Johns Hopkins University, who introduced the distinction—and the word *gender*—in assigning a gender to intersexed children in the 1950s.³⁸

In 1931, on her way to New Guinea, Mead met a "lady reporter" in Sydney, Australia, who used the term *gender consciousness*. The term intrigued her, for it seemed to describe what she intended to investigate (Mead 1977b, 108). Yet she did not adopt it. Instead of *gender*, she used the term *sex*—to refer both to the social and biological constructions of the self. We use the term *gender* to mean the social self; Mead used the terms *sex role* or *sex personality*. Those terms, less precise than *gender*, were rooted in biology.

Following Benedict, Mead concluded: "We are forced to conclude that human nature is almost unbelievably malleable" ([1935] 1963, 280). Then she backed off, suggesting some sort of genetic influence: "If human nature were completely homogeneous material . . . deviant individuals wouldn't so often appear" ([1935] 1963, 283). Then she backtracked: "Let us assume that there are definite temperamental differences between human beings which if not entirely hereditary at least are established on a hereditary base soon after birth. Further than this we cannot go at present" ([1935] 1963, 280, 284). She does not make clear what she meant by "temperamental differences established on a hereditary basis soon after birth"; she may have been referring to the thesis advanced by

³⁷ Mead, "Preface to the 1950 Edition" (1935) 1963, n.p., 1977c.

³⁸ The Oxford English Dictionary dates the introduction of *gender* as a category distinct from *sex* to Oakley 1972. Oakley traces it to John Money of The Johns Hopkins University. Kessler and McKenna (1978, 7) find it in use by the late 1970s.

her friend, psychologist Gordon Allport of Harvard, that personality emerges in the newborn infant only after the "original stream of activity [within the newborn infant] meets the environment, acting upon it and being acted upon" (Allport 1937, 122).

In *Patterns of Culture*, Benedict proposed that cultures select their patterns from social, religious, economic, and individual impulses existing along an "arc of personality," in a process that resembles the development of architectural styles. Redrawing Benedict's "arc of personality" more around individual impulses than cultural ones, Mead also used an argument that might be characterized as "aesthetic." She wrote, "It is as if we had represented the Arapesh personality by a soft yellow, the Mundugumor by a deep red, while the Tchambuli female personality was deep orange, and that of the Tchambuli male, pale green." Yet if we peer more closely, she continues, we see, just as in the spectrum of the rainbow, "in each case the delicate, just discernible outlines of the whole spectrum" ([1934] 1959, 283). This spectrum is the range of individual differences in temperament from which each culture selects its dominant personality type and then enforces that type on its members. How "rainbows" and "colors" are related to the biology of human beings Mead does not make clear.

Indeed, Mead avoided the major criticism of *Patterns of Culture* that emerged in discussions that she, Fortune, and Bateson had had in Tchambuli about the nature of individuals and societies. Benedict had established to their satisfaction that societies were composed of dominant "patterns" under which some individuals became deviants, but she had not adequately explained what produced her "abnormals" in the first place. Moreover, Benedict was convinced that the number of deviant individuals in any society was small because of the power of cultural conditioning over individuals. In contrast, Mead and Bateson thought that there were a lot of deviants in most societies. To explain the genesis of these individuals, Mead devised her thesis that people's lack of recognition of their bisexuality produced widespread social neurosis, and she also looked for systemic relationships among individuals, cultures, and temperaments. Mead later explained that "Benedict believed in temperament [genetic makeup] to a degree, but she never had any theory of temperament" (1977c, 179-92). Together with Bateson, Mead tried to construct such a theory. To do so they turned to speculating about "constitutional types," and that endeavor brought them into conflict with current thinking about race.

Race and constitutional types

Benedict opened *Patterns of Culture* with an attack on racism. Yet she did not include the subject of race in the rest of the book. Similarly, Mead did not refer directly to the topic of race in *Sex and Temperament*, even though she was writing about racialized societies—the word *Melanesia*, referring to New Guinea and its offshore islands, means black. That omission, however, stemmed partly from the approach of Franz Boas, the greatest academic antiracist writer of the age, who was a mentor to both Benedict and Mead. Boas's strategy was to ignore the subject of race in doing ethnography as a way of disassociating culture from race and thus viewing societies from a "color blind" and nonracist point of view (Stocking 1968, 214–19). As Benedict put it in *Patterns of Culture*: "Bodily form, or race, is separable from culture, and can for our purposes be laid to one side except at certain points where for some reason it becomes relevant" ([1934] 1959, 13–16).

Throughout her career as an ethnographer—from 1925 on—Mead specialized in the South Pacific, in cultures that were racially mixed. The steamships from the United States to that region went through Hawaii, and Mead usually stopped in Honolulu and visited friends there on her way to do fieldwork. By the 1920s, Hawaii was becoming as ethnically mixed as it is today (Montagu 1948). Even in Melanesia Mead found a variety of physical types. In an article on racial variation in Melanesia, she wrote about people with light brown skin, such as the Trobrianders, and people whose skins were blue black, like men in the Northern Solomon Islands. She described the people of the Natty Islands as having wavy hair, while the hair of the Fijians was extremely kinky. Six foot tall men were common in New Guinea, she wrote, as well as people as small as pygmies.²⁰ And in *Male and Female*, she wrote that even in the most inbred and isolated groups "marked differences in physique and temperament will be found" (1949, 183).

Such an analysis reflected the principle of "overlapping" in the thought of the Boasians, that throughout both prehistory and recorded times populations shifted and expanded because of migration, wars, and trade, producing a good deal of racial mixing. Above all, interbreeding obscured the lines of racial demarcation. The number of intermarriages in one generation might be small, wrote Boasian Alfred Kroeber, but over time, under the principle of variation by which the laws of Mendelian inheritance

²⁰ Mead, typescript, "People of Melanesia and New Guinea, 1946," LC, I-31.

operated, the descendants of those intermarriages would have a huge impact (1923, 36).

The Boasians also believed that in terms of measuring race according to a scale, most individuals stood in the middle, not at the extremes. These ideas influenced Mead in crafting her theories about gender in *Sex and Temperament*, in which she viewed human identity in terms of a variety of positions on an "arc" of gender and sexuality, akin to the "arc" of race. That does not mean that she abandoned the concept of race any more than she abandoned *male* and *female*. Yet Mead had no difficulty identifying herself with the racial cultures she studied: she thought her personality mirrored that of the Arapesh, while she identified Fortune with the Dobus and the Mundugumor, for masculine and feminine cut across racial divisions, as did reproduction.

In the "Summary Statement of the Problem of Personality and Culture," Mead referred to "constitutional type theories." Indeed, in the letters that Mead and Bateson wrote to each other in 1933–34—when Bateson was in Cambridge and Mead was in the United States, waiting for Fortune (also in England) to sign the papers for their divorce—they seem fascinated by these theories, which cataloged humans by the size and shape of their bodies. By the early 1920s, Mead, like most Boasians, had rejected the popular IQ testing and the tests that measured the size and shape of the brain as ways of ranking races and ethnicities because they had turned out to be biased and imprecise. Yet Mead was devoted to the basic scientific principle of classification. She did not like the types that Benedict used in *Patterns of Culture*, such as Dionysian and Apollonian, but she accepted the concept of typologies. "Constitutional type" theories offered her a way to construct a theoretical grid, to create a science of society.

No recent historian has studied the "constitutional type" theorists, but they were a large group. Classifying people by height and weight went back to the ancients; it was related to phrenology and physiognomy, popular in the nineteenth century. (The former classified personality by the configuration of the skull, the latter by facial features.) In 1951, anthropologist J. M. Tanner traced the genealogy of a sizable group of modern constitutional type theorists to an Italian school and a French school, both of which began in the 1890s. Tanner called classifying individuals by body build "differential anthropology." Some of its adherents took measurements—an approach called anthroposcopy. Others used observation, often of photographs—called anthropometry. In an era in which scientists knew that genes and hormones partly determined body functioning but did not

know how they operated, the "constitutional type" indices seemed a visible manifestation of these hereditary impacts.⁴⁰

The most important of the constitutional type theorists in the 1920s and 1930s was Ernst Kretschmer of Marburg, Germany, a psychologist who studied detailed photographs of the unclothed bodies of 260 psychotic male patients to conclude that manic-depressive disorder correlated with individuals with stocky bodies (he called them *pyknic*) and that schizophrenia correlated with long and lanky or tall and muscular individuals (he called the first type *athenic* and the second *athletic*). He then maintained that these types could be applied to individuals in general to classify individual personality. He denied that his types correlated with race, but when Adolf Hitler came to power, that caveat was overlooked. It was simple to redraw Kretschmer's athletic types as superior beings and to link them with white "Nordics."⁴¹

In their conversations in New Guinea, Mead, Bateson, and Fortune had begun to construct a "constitutional type" theory, as they realized that cultures, like individuals, had specific styles (or patterns), with the Arapesh gentle, the Mundugumor ferocious, and the Tchambuli mixed. They speculated about the relationship of these patterns to individuals and societies and fixed temperamental qualities. They devised an elaborate preliminary scheme called "the squares" that used the points of the compass as markers for individual and cultural regularities. Yet Mead did not refer to the scheme in *Sex and Temperament* since, with the rise of Hitler, such theories could be identified as racist.

Indeed, Hitler assumed power in Germany in January 1933, when Mead first arrived in Tchambuli. By the time she came back to New York City in August, he had abrogated civil liberties and purged the left wing of his party; his alarming ideas about Nordic superiority were being broadcast worldwide. "The Nazi period," Mead wrote in 1968, "was a deterrent to research on race differences and even proved a deterrent to research on constitutional differences and national character in which race was not mentioned. The belief was that research on any kind of difference that might be innate was liable to be misused" (1969, 3-9).

Even in *Male and Female* (1949, 394-95), Mead included a long foot-

⁴⁰ Tanner 1953, 750-70. Roback 1952 identified constitutional type theories as the day's major theory about temperament.

⁴¹ Kretschmer 1970. In the 1940s, W. H. Sheldon studied the photographs of four thousand nude Harvard undergraduates and classified them as *mesomorph*, *endomorph*, and *ectomorph*—a variation on Kretschmer's athletic, fat, and thin. His classifications were popular during my childhood in the 1950s. See Sheldon 1940.

note about the dangers of using constitutional type theories because they might be thought racist. As Bateson pointed out, however, they could become a radical vehicle if the principle that individual variation was more important than sameness were applied (1936, vii). Yet, as Mead implied in her 1968 statement, race was not a category in her constitutional type theorizing, which focused on the physiological characteristics of body height, weight, and mass and emotional qualities such as responsiveness to others. Mead never worked the theory out completely because, as her marriage to Bateson fell apart during World War II, she largely abandoned the project, which they had intended to do together.

The outlines of the project are apparent in *Sex and Temperament*—in Mead's characterization of her societies as conforming to a single pattern but with individuals in those societies displaying a muted individuality in terms of temperamental types. It is also there when she notes that societies can choose strength and beauty as dominant cultural themes. It is present when she describes the Arapesh as slight, small-headed, and only sparsely hairy, with a physique not more feminine than that of the Mundugumor or the Tchambuli. She was working with constitutional type theory when she described the negative attitude of the Tchambuli toward one of their members who most approximated the Western ideal of male appearance and behavior: well-built, with a handsome face, "violent, possessive, positively and aggressively sexed" ([1935] 1963, vi, 11, 149).

In fact, her theory of "constitutional types" illustrates how innovative her biology could be. It also elucidates the ways in which she reconciled her rejection of the masculine-feminine binary with her adherence to *male* and *female* as fundamental categories of human nature. In a statement in *Male and Female* that is focused on constitutional types, she proposed reversing centuries of Western adherence to hierarchical ideals of masculine and feminine appearance based on the superiority of light over dark, youth over age, and height and muscularity in men over smallness and delicacy, with the latter qualities the ideals for women. Long before radical feminists made the point in the 1970s, Mead realized that Western culture uses standards of appearance, based on the masculine-feminine binary, to enforce gender oppression. In her scheme, the basis of classifying the appearance of male and female would be radically different, leading to the disappearance of the binary. Small men would be classified with small women, fat men with fat women, and domineering men with domineering women. The major distinction left between men and women would be pregnancy and childbearing. She wrote:

The fiery, initiating woman would be classified only with fiery, in-

itiating men of her own type. . . . When the meek little Caspar Milquetoast was placed side by side not with a prize-fighter, but with the meekest female version of himself, he might be seen to be much more masculine than she. The plump man with soft breast-tissue, double chin, protruding buttocks, whom one has only to put in a bonnet to make him like a woman, when put beside the equally plump woman will [have a masculinity] still indubitable when contrasted with the female of his own kind. . . . And the slender male and female dancers, hipless and breastless, will seem not a feminine male and a boyish female, but male and female of a special type. (1949, 135)

In this passage Mead suggested that biology could be transformative, a force for gender change. Feminists realized that Mead's cultural relativism provided "new ways of seeing," as Helen Fisher has recently asserted (2001). Yet, because they were focused either on Mead's cultural relativism with regard to gender or her conservatism about women's role, they did not realize that Mead's "new ways" of seeing were biological as well as cultural. As Mead wrote in the preface to the 1950 edition of the work: "The biological bases of development as human beings, although providing limitations which must be honestly reckoned with, can be seen as potentialities by no means fully tapped by our human imagination" (1950, n.p.).

Indeed, her argument about the importance of temperamental qualities disconnected from gender could even be extended to challenge classifying hormones as "male" and "female." Those are on one level semantic categories, embedded in the English language, which, despite the gender neutrality of its nouns and verb endings, still adhere to the concept of a polarity between *male* and *female*.⁴² Following Mead's lead—and given the variety in the individual expression of sex and gender—one might abandon referring to them as "male" or "female" hormones.

Yet after proposing in *Sex and Temperament* that "masculine" and "feminine" are social constructions, toward the end of the book Mead treated them as innate. She referred positively to "essential femininity and masculinity" ([1935] 1963, 321). She could not overcome her fear of being thought a man under the traditional male-female binary or her need to convince Bateson—and the other men she knew—of their masculinity. Drawing on her love of dressing in feminine styles, she wrote: "Just as a

⁴² For a critique of the gendered categorization of the "sex" hormones on the part of a present-day feminist sociobiologist, see Angier 1999, 193–210.

festive occasion is the gayer and more charming if the two sexes are dressed differently, so it is in less material matters" ([1935] 1963, 316).

With regard to men she also found it impossible to discard cultural imperatives for them to be aggressive and masculine. As much as she praised the gentle Arapesh males, for example, she criticized that culture for going too far in scripting the male role as maternal. For she maintained that some "assertive" elements of maleness were necessary to ensure social progress. In *Male and Female*, she issued an additional caution about the Arapesh: "Too vivid an appreciation of the rewards of receptivity are incompatible with adult male roles and may even lead to inversion" (1949, 68, 74). In other words, if men and women were defined as too similar, men might reorient themselves to their feminine side and become entirely homosexual.

Mead also asserted that "a sacrifice of distinctions in sex-personality may mean a sacrifice of complexity"—and complexity was central to those societies that, like the United States in her view, could serve as a model for social change because their ethnic and regional variety produced innovation. And if women took on the personality of men, their contrasting contributions in terms of gentleness and maternal emphasis (as in opposing war) might be lost. In a society without sex distinctions, she concluded, there would be no individuals unadjusted in terms of their "psycho-sexual role." Yet with them would disappear "the knowledge that there is more than one set of possible values" ([1935] 1963, 313, 316). Having rejected the value of homosexuality as identity throughout the book, she reversed this position at its end.

In *Male and Female*, she added caveats to her revolutionary biology. For she concluded that there were "male" and "female" versions of the differing temperaments, as in her men and women with "fiery temperaments" and her male and female versions of the hipless and breastless dancers. She could not entirely break up the categories of "masculine" and "feminine"; they reappeared as entities somehow attached to human genes and hormones. She stated in a draft chapter of *Blackberry Winter* that she, Fortune, and Bateson had agreed on this point in their 1933 discussions in New Guinea, but she only hints at it in *Sex and Temperament*.⁴⁵

In *Sex and Temperament*, Mead brilliantly criticized and revised Benedict's *Patterns of Culture* while remaining faithful to her friend. Daughters disagree with mothers; students both incorporate and challenge their mentors' ideas. Yet Mead preserved their partnership by using Benedict's configurationist approach and agreeing with her cultural relativism. Still

⁴⁵ BW draft, "Sex and Temperament," LC, 32.

she revised her mentor's conclusions with regard to homosexuality, masculinity, and the influence of heredity.

Benedict had established that individuals are patterned by their cultures and that the "deviancy" of homosexuals and the mentally disturbed is interpreted differently in different cultures. Mead clarified those ideas by analyzing the process by which cultural patterning works, especially in the areas of gender and sexuality. Yet despite her argument for an innovative sexual identity, she could not go beyond the boundaries of her culture and her relationships with men to craft a truly revolutionary solution. Thus she eliminated *masculine* and *feminine*, but she failed to cut herself loose from *male* and *female*. She envisioned a progressive future in *Sex and Temperament* but not one in which procreation might be structured in a new way.

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Introduction: Feminism Inside the Sciences

This cluster of articles engages in what I hope will become a growing trend: scientists encouraging and discussing feminist changes in their particular fields of research.¹ The articles presented here address to what extent feminism—conceived as a political and social movement and as an academic perspective—has brought new questions and priorities to the sciences. Where have changes been deep and enduring? Where do problems remain? How can feminists best foster creative and socially responsible science?

Historians like to date the origins of modern feminist science studies (vs., say, contributions by Christine de Pisan or Christian Harless) from the publication of Carolyn Merchant's *The Death of Nature* (1980).² Over the past twenty-some years, a great deal of work has been done on women's exclusion from science, how gender has been one potent factor structuring scientific institutions and practices, and how gender hierarchies have shaped scientific priorities and theories. Much of this work has been done by his-

My thanks to the editors, Sandra Harding and Kate Norberg, who not only encouraged the collaborative work that appears in these articles but whose individual scholarship helped build the base of feminist theory that undergirds our current labors. They and the *Signs* staff moved things along with great efficiency and humor, for which I am grateful. My thanks to the authors—Amy Bug, Meg Conkey, and Patty Gowaty—for taking seriously my questions to them and providing the sustained and nuanced observations that appear here. My contribution to this project is based on work supported by the National Science Foundation under grant 0114706. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the author and do not necessarily (but, in fact, should) reflect the views of the National Science Foundation.

¹ There is a long tradition of working scientists who have brought feminist insights to bear on their work and also contributed to key aspects of feminist theory and practice. See, e.g., Fausto-Sterling 1985; Bleier 1986; Fedigan 1997; Zihlman 1997. There are also luminaries such as Evelyn Fox Keller who left scientific research when they turned to feminist analysis of science. See Fox Keller 1985.

² De Pisan (1405) 1982; Harless 1830; Merchant 1980. It is not my purpose here to review feminist science studies; I have offered an overview in my recent book, Schiebinger 1999. See also Fox Keller and Longino 1996; Creager, Lunbeck, and Schiebinger 2001; Lederman and Bartch 2001; Wyer et al. 2001.

torians, sociologists, and philosophers—scholars who for the most part are outsiders to the workaday world of the natural sciences. By contrast, the articles presented here are by working scientists—Margaret Conkey is an archaeologist; Amy Bug, a physicist; and Patty Gowaty, an evolutionary biologist—who discuss in concrete terms ways in which feminism has changed or has failed to change each of their disciplines. Here we see scholars grounded in feminist studies of science working inside the sciences. It is these scholars who are particularly well positioned to embed gender analysis in basic research and train the next generation in these methods.

One purpose of this cluster is to foster closer relations between feminist science studies and the sciences. Feminist science studies has followed a path similar to that of its parent fields, science studies and the history and philosophy of science: each has developed as an exacting discipline of its own, its insights and methods having made few inroads into the sciences. Why this is so needs to be fully explored and widely discussed. In part, collaboration between humanists and natural scientists has been limited because many scientists—as each author here discusses—think of their work as “objective,” existing apart from social relations. In part, collaboration became even more difficult when, in the late 1980s and 1990s, certain factions within science studies began to practice intemperate constructivism, to the extent that scientists’ distrust of scholars examining their disciplines escalated into the “science wars” (Gross and Levitt 1994; Ross 1996). As Conkey notes, many “objectivists” in the debate simply assumed that feminists belonged to the “anything goes” relativist camp. Drawing on philosopher of science Alison Wylie’s work (1992), Conkey describes how many feminists have developed a “strategic ambivalence” that “refuses reductive constructivism as firmly as it rejects unreflective objectivism.” Feminist archaeologists in this tradition recognize the ambiguity in data and the existence of sociopolitical factors while still respecting empirical constraints.

The articles presented here give a sense of how difficult it is for scientists to incorporate insights from other disciplines into their own.³ One of the contributors told me of furtive moments “stolen from science” to write an article for *Signs*—a publication that would not have “counted” for tenure or promotion in her field. Another told of learning feminist analytics on the side as an extra burden in an already full schedule. Another noted that feminism is still not a welcome congener to science in many of her professional circles: “It has not been easy,” she emphasized.

³ For a discussion of the problem of working across the women’s studies/science divide, see Mayberry, Subramaniam, and Weasel 2001, sec. 1.

Establishing niches for feminism in science has been difficult, but establishing science inside bulwarks of feminism has been equally arduous. As Conkey pointed out to me, hers is the first article on archaeology ever to appear in *Signs*, a journal that prides itself on being interdisciplinary. At the same time that we encourage migration of feminism into science, we need to continue our efforts to breed science studies inside feminism. Amy Bug, in her contribution, discusses how women's studies programs, usually dominated by humanists and social scientists, can be intolerant of suggestions (e.g., for campus events) from scientists because these may not be consistent with "cutting-edge" feminism. I would also second Conkey's call for continuing efforts to mainstream feminist-inspired natural science into feminism. This cluster demonstrates the kind of creative ferment that collaboration across the humanities and the social and natural sciences can achieve.

One thing to consider while reading these articles is the kinds of intellectual tools these scholars employ to promote feminist research. In the past, feminists have attempted to create feminist methods and epistemologies from feminist values. And, indeed, our research embodies many core feminist values: Conkey summarizes them here as democratizing research, eliminating research that leads to exploitation of nature or other humans, resisting explanations stripped of social and political context, and so forth; and Bug offers Ruth Bleier's list, which includes acknowledging our values and beliefs, being honest about our assumptions, being responsible in our language, and so forth. One aspect worth noting in these articles is the nonuniqueness of much of the feminist research discussed. In fact one sees a tight interlacing of feminist research programs, first, with very standard methods of research, such as a continuous critique of foundational assumptions and a relentless search for alternative hypotheses, new perspectives, and insights; and, second, with other socially responsible science, such as attention to social priorities in setting research agendas and attention to who benefits in terms of wealth and well-being and who does not from particular research programs (Harding 1991).

What we see in these articles is that methods used by feminists are as diverse as the variants of feminism and of science itself. Gowaty, for example, makes great headway fighting sexist science with good old-fashioned empiricism. She is a tough thinker, an ace researcher, and a formidable opponent with her hands on the data. As she has pointed out to me, no one—scientist or otherwise—likes to be criticized: the best way to change a particular aspect of science is to do it better than those you wish to criticize; that is, to incorporate pioneering insights into a field using the "language, tempo, mode, and methods" (however modified) acceptable to that par-

ticular field.⁴ These articles demonstrate a core value of flexibility: as with any set of tools, feminist tools must be newly fashioned and reworked as circumstances change.

But what then is uniquely feminist about the research discussed in these articles? Both Conkey and Gowaty raise the disturbing point (which others of us have seen in different contexts) that making females visible and raising questions concerning gender can be done without feminism.⁵ There are, however, distinctive aspects of feminist science studies. Conkey has called what she does "doing science as a feminist" (see her article). Doing science as a feminist means mainstreaming politically engaged gender analysis into all aspects of science—its institutions, theories, practices, priorities, and policies.⁶ Doing science as a feminist means, as Gowaty phrases it, "to modify the rhetoric of the women's movement into stable hypotheses" for use in science. Feminists, then, employ the full range of methods, insights, and creative sparks available to them as scientists and as feminists. Finding evidence in the laboratory or field may be done with well-worn research methods—but put to new ends. New questions about old assumptions often lead to the development of new techniques and improve the overall design of research, as these articles detail.

A word of caution: it is an interesting phenomenon that when feminist insights become mainstreamed in a science they are sometimes thought of simply as "good science." And perhaps they simply are. This has the effect, however, of keeping things labeled "feminist" always on the radical fringe. This is one reason some people—even those who support women and feminist scholarship—shy away from labeling themselves as feminists (something Conkey discusses in her article here). It is important that we

⁴ Private communication.

⁵ Since the late 1980s, *gender* has become a polite word for feminism. "I do gender studies" is more acceptable in the academy than "I am a feminist." *Gender* has also become widely used in the United States and increasingly in Europe as a polite word for sex. The National Science Foundation (NSF), e.g., asks for a principal investigator's "gender." It was heartening to see an article titled, "The Use of 'Sex' and 'Gender' to Define and Characterize Meaningful Differences between Men and Women," as the lead article to the National Institutes of Health's (NIH's) Agenda for Research on Women's Health for the 21st Century (NIH 1999), vol. 1, 15–19.

⁶ See the conclusion of Schiebinger 1999. "Mainstreaming" gender into science is also the basic strategy for reform in the European Union as set forth by the Women and Science Unit, founded in 1998. *Mainstreaming* is defined as the systematic integration of equal opportunities for women and men into scientific institutions, their cultures, and into all programs, policies, and practices: "Into ways of seeing and doing." See Rees 1998; European Technology Assessment Network (ETAN) on Women and Science 2000.

recover insights now considered simply "good science" or "science as usual" that grew out of feminism.

It will be obvious from this set of articles that the effects of feminism have not been uniform across the sciences. While feminism is making good headway in archaeology and many areas of the life and human sciences where objects of research tend to be sexed and gendered, it is lagging in the physical sciences. As Amy Bug's article on physics shows, however, more people, now trained in physics and in feminist studies, have taken up the cause. As Bug discusses, many mainstream physicists today are willing to agree that more women need to become physicists, but few accept the notion that feminist insights are useful in describing the fundamental laws of the universe. Drawing from a wide range of scholarship, Bug offers a valuable assessment of where feminist methods converge with those of conventional physics research and pedagogy, and where they do not.

For momentum to continue in feminist studies of science in the coming decades, many developments are desirable. Let me highlight four. The first is a closer collaboration between humanists, social scientists, and natural scientists as highlighted in these articles.⁷ A second desired development is for both humanists and scientists to provide science students with basic training in science studies, of which feminist analytics is an important component. While most people agree that a student needs to learn biology or physics in order to excel in those fields, many believe that he or she can just "pick up a critical understanding of gender along the way." Understanding gender dynamics in science, however, requires research, development, and training, as does any other field of intellectual endeavor. Success in this second desired development might lead to the third: making feminist analysis something more men embrace.⁸

Finally, policy reform is required to persuade the NSF to follow the NIH's lead in mandating that gender analysis become a required element in basic research. The NSF has limited its programs designed to deal with the shortfall of women scientists to attempting to increase the supply of qualified women by promoting women's opportunities in science at all levels and by addressing gender issues in some curricular reform. By contrast, the NIH Office of Research on Women's Health, founded in 1991,

⁷ As a small initiative, I will be organizing a workshop funded by the National Science Foundation (2004) on how working scientists—both men and women—have employed feminist analytics to open new research questions and perspectives.

⁸ A number of men have contributed substantially to the feminist critique of science; one thinks of Scott Gilbert, Carol Purcell, and those mentioned in the articles here.

took the necessary (in my view) step of joining increased opportunities for women to mainstreaming gender analysis in medical research (Pinn 1991). Reforms at NIH worked because a solid body of research, along with well-honed tools of analysis, were available to be plugged in from leading women's studies programs at universities around the country. Research from these programs insisted from the outset that gender intersects in vital ways with race, ethnicity, class, sexuality, disabilities, and so forth (as also discussed in this cluster). The approach of NIH has not only increased the number of women (of many backgrounds) in the medical sciences but has also wrought significant improvement in U.S. medical bioresearch and health care (Mastroianni, Faden, and Federman 1994; Ruzek, Olesen, and Clarke 1997; NIH 1999).

Some will object that NSF does "basic" research, different in kind from that pursued at NIH. But, as this cluster of articles demonstrates, gender bias is as real in basic research as in applied research. The NSF currently requires all applicants requesting funding for conferences and workshops to address the "support and recruitment of underrepresented groups" in their proposals (NSF 2001, iii). This requirement should be given intellectual breadth and bite to ask: How will gender analysis figure in the plan of all basic research?

To return to the question at hand: Has feminism changed science? The articles in this cluster answer with a resounding "yes." Conkey, Bug, and Gowaty each document the ways in which feminists have posed new conjectures and devised new research strategies in their particular fields. Yet each author follows the "yes" with a "but." The process is well underway but must become more widely practiced and be ongoing. What is exciting about this set of articles—in contrast to the situation, say, fifteen years ago—is how embedded this process now is inside the sciences.

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Has Feminism Changed Archaeology?

Fortunately, I am addressing this question—has feminism changed archaeology?—in 2002 and not in 1985, for the past fifteen-plus years of work in feminist-inspired archaeology have been explosive.¹ I will take up the topic at hand as follows: First, I will give a brief background on archaeology as a discipline, especially in North America. Second, I will make a preliminary interrogation: have the questions and concerns of feminism and feminist archaeology had any impact on the particular *kind* of archaeological work that is being done? Here I will discuss at least three major directions. Third, I will refer to the epistemological implications of feminist influences and practices in archaeology and then follow this by noting convergences that have helped support the directions that feminist archaeology has been advocating. Finally, I will offer something of an assessment: what are not only the gains but the remaining or new issues, problems, and implications?

Who are archaeologists in North America?

Most archaeologists in North America are members of two primary professional organizations with somewhat overlapping memberships, the Archaeological Institute of America (AIA) and the Society for American Archaeology (SAA). The AIA membership includes a wider range of archaeologists and is the primary archaeology home for those with more humanistic and history backgrounds, including most of the classicists, and for those who study major so-called civilizations (Egypt, Rome, Greece,

I am grateful to Linda Scheibinger for inviting me to join this group of commentators and for her enthusiastic inclusion of archaeology in her vigilant scrutiny of the sciences. It goes without saying that I have been especially blessed with so many feminist colleagues in archaeology at Berkeley, and elsewhere.

¹ An article such as this would, of course, not be possible without all the work done by so many feminist scholars in archaeology and other fields. Given the strictures on space and references, I am both grateful and beholden to those whom I have not been able to reference in the article, but whose contributions have been enormous

the Maya, etc.). Members of the SAA are mainly anthropologically trained archaeologists who are in anthropology departments; in federal, local, and state management; and in cultural resources work; or are avocational archaeologists with interests in the Americas. While all archaeologists increasingly use scientific techniques (e.g., chronometric dating, ground-penetrating radar, Landsat photos, soil chemistry), AIA members tend toward more humanistic and text-aided archaeologies practiced quite globally, whereas SAA members are more anthropological, and the majority practice in the Americas. This anthropological archaeology, which emerged in the 1960s (often referred to as the “new” or “processual” archaeology; see, e.g., Johnson 1999), brings with it a certain intellectual history that is marked, in part, by a strong and explicit (but sometimes confused) identification with science and scientific method. Although the scientism of this archaeology has been challenged, especially since the 1980s, and although there is even a small sector of archaeologists who are out-and-out constructivists—“the past is purely a construction in and of the present”—it is safe to say that most archaeology being practiced in North America is in the intellectual heritage that considers archaeology to be a scientific enterprise strongly linked to empirical phenomena: “it is through the material culture of the archaeological record that we infer the past.”

Thus it may well be that the rise and perpetuation of an explicitly scientific archaeology and a purportedly objectivist approach well into the 1970s (and since) have been central to why anthropological archaeologists were not very influenced by nor engaged with the vibrant and pioneering feminist and gender anthropology that characterized 1970s sociocultural anthropology (e.g., Rosaldo and Lamphere 1974). Other factors were surely at work as well, including the relatively small number of women archaeologists, especially in visible, respected positions (Wylie 1991). Overall, feminist interests and even gender research came very late to anthropological archaeology.

A few more observations are relevant about the practice of archaeology as a field-based and international discipline. The practice of archaeological fieldwork has long been male dominated (Gero 1985, 1994), and much has been written about this with accompanying statistics to show how the “big digs” have been primarily male led—“the practicing field archaeologist who himself conquers the landscape, brings home the goodies, and takes his data raw!” (Gero 1985, 344)—and how males have received more funding for such excavation than have females (see Moser 1996). Not only has fieldwork, and excavation at that, been gendered male, doing excavation (as opposed to other kinds of research such as survey research

or the analysis of museum collections) and having one's "own site" have been privileged as central to the crucial emphasis on fieldwork, as to what defines a "real" archaeologist. This is a rich topic to be mined in relation to feminist concerns.

Second, many of us in the United States, as well as archaeologists from other countries, "do" archaeology in places other than our or their own countries; most of these other countries have different research traditions, intellectual and scholarly paradigms, and cultural constraints and expectations that are also gendered (e.g., Nelson, Nelson, and Wylie 1994) and sometimes gendered differently. And there is at least another entire paper to be written on the fact that in the past fifteen years some distinctly different approaches to feminist and gender archaeology have evolved in different research traditions (e.g., compare Conkey and Gero 1997 with Sørensen 2000). Many European feminist archaeologists, for example, consider their work to be more concerned (than is that of North American gender archaeologists) with such issues as identity construction, life cycles, and the body. They would suggest that many North Americanists have focused more on gender roles, labor, and status and have remained more under the sway of their scientific archaeological legacy (Gilchrist 1999).

The feminist impact: At least three directions

I have tried to give a sense of some of the contexts of archaeological practice and the many dimensions in which a feminist critique has been called for. In fact, there now exists a vibrant literature that addresses gender and feminist issues in and for archaeology. Have the questions and concerns of feminist archaeology had any impact on the kind of archaeological work that is being done?

Not surprisingly, one of the first concerns of feminist-inspired archaeology has been to make the women of the past visible, particularly once archaeologists came to see the highly androcentric accounts of the human past that prevail (Conkey and Spector 1984). This research and inquiry has had enormous success, but, perhaps, at some cost. From the mid-1980s and into the present, at least 80 percent of the literature that one might consider under the rubric of feminist archaeology in its broadest sense is literature about women, females, and recovering and "finding" women and gender relations. A fuller account of this phenomenon would have to take into consideration wider social science-related discussions of how gender came to take the center stage, what kind of a concept it is, how it is problematic in our international communities where there is no comparable term (e.g., in Spanish or French), and how gnarly a concept

it is when considering human societies that existed thousands and tens of thousands of years ago. Problematically, to some of us, gender has been studied without engagement with feminist resources (see Hanen and Kelley 1992; Wylie 1996; Conkey and Gero 1997). In fact, while there are many books on and adherents to gender archaeology,² these are far more visible and numerous than those explicitly taking up feminist archaeology.

Alison Wylie (2001) has made the case that gender-focused archaeology, and one in which women and gender are made visible, is certainly well within the intentions of feminist work: that is, to be sure that gender is not "disappeared" (after Longino 1994). One central example of how this interest in women and gender has led to innovations and new directions in research would be the research into past cultural situations (or so-called contexts) in which women were likely to have been present or in which gender was "at work." Here both these very contexts (such as household labor) and related classes of information and archaeological artifacts (such as spinning, weaving, and net making) have been explored, usually with more intensity, scrutiny, and methodological innovation than ever before. There is no doubt that there is much more work at what we might call the microscale, at the level of the household or the event where daily practices, structured space, local knowledge, and local production, for example, are both more accessible to the archaeologist and have been taken up with new vigor and success, motivated in large part by a concern for women and gender (see, e.g., Tringham 1991).

While we want to understand past life at both the macroscale—such as trading networks, sociopolitical alliances, demographic trends, and so on—and at the microscale—such as households, and especially the potential and actual productive practices of women and children—the latter has been, until recently, relatively ignored. This has been especially the case given the macroscale, "big systems" concerns of the still dominant theoretical framework of "processual archaeology." However, along with increased interest and attention to the microscale come both additional theoretical resources (e.g., Joyce and Gillespie 2000 on house societies) and methodological developments, such as highly developed micromorphology and soil chemistry to infer microscale practices from such things as analyses of deposits (especially on house floors) and more detailed architectural histories that look at the life cycle of a "house" or structure more than at its static form at any one time. Overall, one major contri-

² Examples of gender archaeology publications would include Wright 1996; Nelson 1997; Hays-Gilpin and Whitley 1998; Sorenson 2000.

bution of feminist-inspired archaeologies has been to emphasize the centrality of a more explicitly multiscalar archaeology.

For example, the usual story about the establishment of Inca political influence in the Andes would stress primarily the macroscale processes of "political expansion," assumed to have been orchestrated and enacted by men. But in a close study of changing spatial patterns of the household-level production of corn, along with bone chemistry analyses of skeletal remains of males and females that allow us to infer their diets, Christine Hastorf (1991) has shown that an integral component to this "political expansion" was a local-level, even household-level, intensification of corn production and processing (which is ethnohistorically known to have been the domain of women) and a shift in dietary intakes. From this evidence, Hastorf has reasonably inferred increased labor by women and increased corn consumption by men in the form of corn-based (*chicha*) beer, as part of requisite political feasts that make manifest Inca political control. Thus, Hastorf provides us with an excellent insight into the entangled relationships between gender, food, and politics.

A second arena that has been the subject of renewed interest and increasing sophistication, thanks in large part to feminist-inspired archaeology, is visual representations (art, imagery, iconography). Of course, here is a domain of cultural production where "women" might be more immediately and convincingly "visible," because they are actually depicted in murals, as figurines, on monuments, or in rock art. Some archaeologists have embarked on a long-overdue consideration of what images, especially of gendered subjects, might mean, drawing in part from art history and theories of representation. As in the wider field of feminist studies, such inquiry often sheds light not just on women and females but also on masculinity, males, and the varied ways in which engendering—as a socially transformative process (e.g., Joyce 2000)—may have been enacted in ancient societies. For example, Rosemary Joyce (1998, 2001) has drawn on the feminist mobilization of performance theory and concepts of performative gender in regard to ancient Mesoamerican images, iconography, figurines, and other material culture. This inquiry allows us to see how concepts of "personhood" were made manifest in differing ancient Mesoamerican societies at varying historical moments, and Joyce shows how detailed analyses of imagery with an explicitly feminist eye can yield insights into aspects of sexuality, homoeroticism, gendered statuses, and symbolic capital in past human societies, topics hardly imaginable to previous generations of archaeologists.

As well, it has been feminist perspectives on the *use* of visual images

in the presentation of archaeological knowledge that have opened up a new field of critical analysis. That is, the visual language of archaeology—our maps, charts, and the artistic reconstructions of scenes from the past—is crucial in how archaeologists convince their readers (Moser 1998). But this imagery is not neutral, especially in regard to the representation of males, females, and gender. For example, surveys have shown that males dominate many scenes of the past (Gifford-Gonzalez 1993) in a way that conveys the idea that they were the ones most responsible for such revered (by us) cultural creations as Ice Age cave art, new technological inventions, and the governance of great civilizations. The visual impact of such scenes, especially in more popular media, has contributed to the legitimization of contemporary, patriarchal notions of hierarchy, value, and essentialized gender roles. These are often justified in the present by references to aspects of the past about which we do not (yet) have any way of knowing who the makers and actors were; for example, we just do not know who it was that painted caves in Ice Age Europe, even if modern-day artists' depictions of Ice Age cave painting "tell us" such painting was done by men only (Conkey 1997).

Third, feminist archaeology has been especially concerned with articulating critical analyses of heretofore assumed and taken-for-granted, many of which can be shown to be highly gendered concerns of the present. For example, we have categorized most of the human past into periods defined by technologies or economies (e.g., the Stone Age, the Iron Age, hunter-gatherers, village agriculturalists); the centrality of tools and technologies in explaining our evolutionary success ("man the tool-maker") is much heralded. However, by not only questioning these assumed centralities and objects of knowledge as being highly gendered in line with contemporary gender ideologies but also pursuing other possible factors and processes that must have been "at work" over the course of human cultural life, feminist archaeologies have expanded the ways in which we might come to understand the long-term changes of human societies as well as the local enactments of particular human societies and even individuals. For example, an archaeology concerned with gender has drawn our attention to such processes as the establishment of social alliances and the social, and differentially gendered, relations of production; to female as well as male initiation sites; and to the importance of gathering, scavenging of kills, and small-game hunting, often the task of women and children, even in so-called big game hunting societies.

That is, feminist approaches have raised questions about our very central operating concepts—technology, inequality, household, hunting, and even gender—which we have previously held as somewhat essentialized or nor-

mativized phenomena. Feminist archaeologies are concerned with the consistent unquestioned linkages of certain activities of the past with each gender (e.g., male activities/female activities) and, furthermore, with assumptions of the very existence of a bipolar, two-gendered world in past societies.

As a result of more than a decade of feminist-inspired archaeologies that have explored new domains and critiqued taken-for-granted, there is little doubt that we are often doing better archaeology. We have begun to expand the range of possibilities as to what happened in the past; we have shown how there are other stories to be told. We have learned, for example, about the role of different kinds of women's labor (textile manufacture, food production) in maintaining the Aztec state (Brumfiel 1991). We have learned about the ways in which boys and girls were differently gendered into the adult world (and in more than two ways) in ancient Mesoamerica (Joyce 2000). We have come to see that the famous female figurines of the Late Ice Age in Europe bear testimony to intricate weaving skills, suggesting that those who were net makers and weavers of knots were also the makers of the figurines (Soffer, Adovasio, and Hyland 2000)—a rather different story from the one usually told about the figurines as "Venuses," made for the male gaze or solely for promoting or revering female fertility, collapsing female value into a biological domain.

Some epistemological implications

Feminist archaeology has been more than fortunate to have at our side an articulate feminist philosopher of science, Alison Wylie, for whom archaeology has always been a major analytical focus. Among Wylie's important observations is the perhaps unique position of feminist archaeologists vis-à-vis the epistemological debates or differences within the discipline. By the 1990s, it appeared as if there were only two epistemic options in archaeology once one recognized the sociopolitical aspects of archaeology, as happened at least to some extent in much of the discipline. On the one hand, there was increasing and explicit acknowledgment that sociopolitical factors matter and that archaeological activity and knowledge embody the situated interests of its makers (e.g., Trigger 1984; Kohl and Fawcett 1996). On the other hand, there has been much—sometimes hostile—denial of this by those who maintain that archaeology is an objective science and any sociopolitical factors are external to the process and its product.

Wylie has effectively argued that feminist research in archaeology is one

area in which advocates have, all along, not opted for one or the other of these two polarized epistemic options. Rather, she suggests that feminist researchers are characterized by a "strategic ambivalence" that "refuses reductive constructivism as firmly as it rejects unreflective objectivism" (Wylie 1997, 81). As such, feminist archaeology has created an alternative epistemic position, even if many of the objectivists would unreflectively (and incorrectly) lump us with the more relativistic "camp." This position of feminist archaeology recognizes both the ambiguity of the archaeological record and the existence of sociopolitical factors and contingencies and yet insists on respecting empirical constraints (see also Wylie 1992): while there may be a number of alternative interpretations, it is not the case that one can say anything!

Furthermore, feminist archaeology has taken up some of the epistemological and therefore procedural mandates of feminist thinking more widely (see Longino 1994 and Wylie 1995, as well as Bleier 1984): to resist single-cause explanations stripped of their social and political context; to eliminate research that leads to/endorse the exploitation of not just nature but of other humans; to democratize research, fostering views from "many wheres"; to be open to continuous revision, in theory, evidence, and interpretation; to favor theories that do not "disappear" complexity and heterogeneity; and, above all, to pursue continuous critical assessment of our key categories of analysis and key presuppositions.

Contributing convergences

There have also been certain converging trends within the field and without that have helped support (and even authorize) the directions that feminist archaeology has been outlining and advocating. For example, in her now classic feminist monograph, an archaeological study of a Wahpeton Dakota village, Janet Spector (1993) fully takes up one of the early concerns of feminists in archaeology, namely, what we might call "peopling the past" (see also Tringham 1991); that is, putting faces on the people of the past instead of talking about the past in terms of impersonalized (if not depersonalized) phenomena such as technoeological strategies, subsistence systems, intensification of agricultural production, and so on. Peopling the past means valuing the experiences and contributions of men, women, and any other genders. In fact, such an approach wants to place human experience and social action at the forefront, which itself forces a scrutiny of assumptions, epistemologies, and exactly what our objects of knowledge are.

On the one hand, in her monograph, Spector shows how the very

classification systems and practices that archaeologists use to deal with the artifacts (e.g., "broken bone technoforms," rim shards) found in archaeological sites are depersonalizing. On the other hand, she shows how effective it can be to create a narrative, even if an admittedly speculative but plausible narrative, that links artifacts (in her specific case study, a bone awl) with a putative owner or user (an imagined young Dakota girl). Here she draws on ethnohistory to make this link, but even if the awl in question were not that of a specific young girl, the effect of the narrative is to bring into focus the experiences of people and their material worlds, and these can be varied and varying experiences.

This kind of "peopling" is in convergence with wider trends in the discipline of archaeology and its sociopolitics of the last two decades that have required archaeologists in most places to confront more explicitly who the stakeholders are in the knowledge that we produce. That is, from other quarters (e.g., federal law, the Native American Graves Protection and Reburial Act), archaeologists are being pressured (appropriately) to be more self-reflexive about the knowledge/stories they tell and to be more responsible to indigenous peoples and descendant communities and to others who may be stakeholders. These pressures very much support and endorse the kind of "peopled past" for which Spector and others argue. And, for Spector, it was the very intersection and mutual reinforcement of her feminist commitments and her sense of responsibility to the descendant (Dakota) communities of the specific archaeological site that provided the crucible within which her monograph was conceptualized and written.

Has feminism changed archaeology?

Absolutely: the archaeological study of gender and the gender of archaeologists are in most introductory textbooks. Gender and feminist archaeology are topics for entries in the current spate of encyclopedias of archaeology and of the social sciences (e.g., Engelstad 2001), and an entire encyclopedia of archaeology and gender is in the works. At least two presses have a publication series on archaeology and gender (University of Pennsylvania Press and AltaMira Press), and in 2000 the very first (but so far, only) academic job ad explicitly for a "feminist archaeologist" appeared in the October *Anthropology Newsletter* (AN).

Yet, in some ways, these are phenomena that can be more or less ignored; passive smoke has had more impact on American social life than passive "acceptance" of archaeology of gender or feminist archaeology has had on archaeological practice. Undertakings in gender or feminist

research are still relatively ghettoized: it is mostly women who "do" it. The research that is being produced is often undertheorized (Conkey and Gero 1997; Bender 2000); that is, "gender" is still often just another variable that has been added to an unreflexive, somewhat positivist approach. Many still proclaim that we cannot possibly "see" gender. Overall, there are a number of crucial issues that other feminist scholars have been working on that are not yet in the vision field of most archaeologists, even those working with gender or feminist concerns, such as the debates over concepts of "difference" and work on the intersectionalities of race, class, and ethnicity (e.g., Collins 1999), as well as with sexuality, although this latter domain has just gotten off to a very exciting start with an edited volume on archaeologies of sexuality (Schmidt and Voss 2000). Here the editors showcase nearly a dozen case studies of how archaeology can contribute to understanding sexuality in the human past: from studies of societies with multiple genders to the historic archaeologies of prostitution and how an archaeological study can yield insight into sexualities within a nineteenth-century women's prison in Tasmania. Despite such work, many models that archaeologists have been using in their enhanced approaches to the microscale are unreflexively Western, normative, and heterosexual.

While there is no doubt that the concept and study of "gender" is visible, archaeology has not engaged critically with what it means to "make women visible" and how we have done so. Many archaeological accounts have provided only an "optical illusion" (after Burton 2000) whereby, yes, there are now women, but in roles, activities, and significances that are unproblematized. As some of us have argued (Conkey and Tringham 1995) in relation to the reinterpretation of ancient female figurines as veritable "goddesses" of a female-centered world, to replace a patently androcentric and totalizing narrative with a gynocentric yet totalizing and limiting narrative is to miss the point of feminist scrutiny. Furthermore, archaeology has not, to any observable extent, critically engaged with what it means to *do* archaeology (whether or not gender is a topic) *as* a feminist; attention to the feminist practice of archaeology is only emergent (Conkey and Wylie 1998); and many aspects of feminist practice (e.g., collaborative, activist, and participatory methodologies) are well suited to archaeology. While most of the archaeology and gender literature has been in edited volumes (e.g., Wright 1996; Claassen and Joyce 1997, among many) or a few single-authored volumes (e.g., Nelson 1997; Gilchrist 1999), to take up an entire monograph-length study from an explicitly and theoretically engaged feminist perspective is still very rare (e.g., Spector 1993; Gilchrist 1994; Joyce 2001), not to mention the fact that articles that

explicitly address gender (to say nothing of feminist archaeology) in the major archaeology journals are infrequent.

Even more important and problematic is the fact that feminist-inspired archaeology has not yet made itself very visible in mainstream feminist scholarly journals and edited collections. There is an absence of (much) meaningful dialogue between archaeological theory and feminist theory, and few feminist scholars are aware of the extensive literature and engagement of archaeology with gender and feminist research.

In archaeology "gender archaeology" has been a productive entry into diverse archaeological topics, even if it has been "contained" (in part, by not being theorized) and the overall effect has been more conservative. Nonetheless, what constitutes (archaeological) knowledge is up for discussion, as well as is *who* can be a knower in the discipline. Given that archaeological knowledge has been a crucial and major contributor to the particular (discriminatory, patriarchal, and essentialized) versions of human life that we now all still exist within, feminist influences in archaeology have certainly begun the necessary challenges of that knowledge and have begun to transform it so as to change these worlds. To the extent, furthermore, that this is being done without our becoming "anything goes" kinds of relativists, feminist archaeology has an even richer future.

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Has Feminism Changed Physics?

Someone once said: "Scientists and prostitutes get paid for doing what they enjoy."

—Stephen Hawking, *Black Holes and Baby Universes and Other Essays* (1993, 16)

Women's work is of a particular kind . . . it always involves personal service. It requires caring labor—the labor of love.

—Hillary Rose, *Sex and Scientific Inquiry* (1987, 275)

In this article, I assess the recent manner in which feminist ideas have contributed to Western physics. This assessment is complicated, for there exists a strong tension between differently aimed critiques of the field. Among practitioners, there is an inclination to distinguish sharply between issues of "physics" and issues of "physicists." Thus, on the one hand, most physicists concede that the gender and racial composition of physics students is in need of balance. This is correlated with a mainstream movement to create race- and gender-friendly "niches" in the university for students who are "different" and with efforts by women physicists to network, mentor, recruit, and retain more women and help each other build healthy careers within the mainstream. On the other hand, most physicists presume that feminist critique is incapable of generating ideas that will make a superfluid colder, a plasma hotter, or a particle beam more intense. This is correlated with the fact that feminism has done essentially nothing to transform what one might call "orthodox physics": the body of laws, the rules of inference that relate laws to consequences,

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and the experimental methodologies by which physicists interact with and build abstractions of the natural world. Finally, most physicists would like to see more young people pursue physics and to have more sympathy for physics from an educated public. This is correlated with the existence of a physics education reform movement, which is thriving. Yet this movement is largely disconnected from the literature on race-, class-, and gender-dependence in knowledge acquisition strategies.

Getting the right number: Education reform

To the uninitiated, demographics is the only issue relevant to women in physics. This does not imply, however, that this concern is foolish or misplaced. It may be that a "critical mass" must be achieved before feminism within the field of physics can mature and proceed as in other fields. To date, women are seriously underrepresented in physics: in 1998, women constituted 51 percent of the world population, and in the United States 21 percent of the bachelor's degree recipients, 13 percent of Ph.D.s, 17 percent of assistant professors, and 3 percent of full professors (the figure did not change in the four years between 1994 and 1998). Women are 40 percent more likely than men to be hired in a part-time job (Ivie and Stowe 2000). Members of underrepresented minority groups are under-underrepresented. In 1997, fewer than 2 percent of physics Ph.D.s went to black Americans. Of these fifteen people, fourteen were men (National Science Foundation 2000). In 1996, Roman Czujko reported that the number of institutions with no women on the physics staff had dropped from 55 percent to (only!) 35 percent in a decade and that essentially no progress had been made in the number of African-American and Hispanic Ph.D. recipients.

In order to recruit and retain physics students, a physics education reform movement has taken hold. The Physical Science Study Committee (established at MIT by Jerrold Zacharias) took up the work in the 1960s. Since 1989, Project Kaleidoscope, a national alliance of scholars, has worked to reform physics as well as other sciences, mathematics, and engineering. In 1999, the American Association of Physics Teachers (AAPT) and the American Institute of Physics (AIP) founded a Task Force on Undergraduate Physics (Hilborn 2000). The goal was a major revitalization of undergraduate physics courses. There are currently at least twenty U.S. graduate programs offering Ph.D.s in the field of physics education research (PER) and thirty-five tenure-track positions in PER for people who specialize in "how students learn physics" (Lopez and Schultz 2001).

All of the notable curricular movements have what we might recognize as "female-friendly" activities (Rosser 1990, 1995). Examples include Eric Mazur's "peer instruction" (1997), in which students interact in small groups and either agree or agree to disagree on answers; Wolfgang Christian and colleagues' "just-in-time-teaching" (Novak et al. 1999), in which students engage in face-to-face coaching on problem-solving skills, professors use their students' own words in lecture, and the program stresses the importance of the professor's learning about the experiences of individual students, not just the class as a whole; Lillian McDermott's "inquiry-based physics" (1996), a program that allows students to begin with their own speculations, make extended observations, and develop their own concepts and models to construct knowledge; and Priscilla Laws's "workshop physics" (1997), which features hands-on activities with both specialized and nonspecialized laboratory equipment and which is predicated on the idea of lecture-free "active learning" and of trusting a student to teach herself or himself additional material. In polling more than a dozen universities that offer advanced degrees in physics education, I found much sensitivity to difference, good will toward women succeeding in physics, and substantial female populations of students and postdocs. In three cases, I was told that issues of women were a side interest on which participants had done research.

In summary, the good news about PER is that many of its axioms are feminist related, for example, that the student body should be more diverse ethnically and economically and that physics should be taught to emphasize its connection to the daily lives of people in our society (Hilborn 2000). On the other hand, the axioms show a very early feminist stage of curricular reform. Sue Rosser (1997) notes that faculty who have initiated programs to attract and retain more science students, including women, typically come from the sciences and typically (though not always) do not have an extensive knowledge of women's studies and feminist theory.¹ For example, we might not hear an explicit mention of the women-and-minorities problem but, rather, a reference to "different types of students," nor would there be explicit mention of historically black or women's colleges but an oblique comment that "departments have different missions" (Hilborn 2000). In a recent article in *Physics Today*, the authors discuss how cognitive science research shaped the

¹ Priscilla Auchincloss, Karen Barad, Angela Barton, Ingrid Bartsch, Kaye Edwards, Anne Fausto-Sterling, Scott Gilbert, Bonnie Schulman, and Bonnie Spanier are just a few of many "full-fledged" scientists with full women's studies credentials who have designed science curricula.

kindergarten–grade 8 (K–8) science education reform movement (Lopez and Schultz 2001), yet there is no mention of the extensive cognitive science literature on how the gender of the child plays a role in learning behaviors.² In a box labeled “Are You Part of the Consensus?” there is a list of items instructors want their students to know and to be able to do. While many of these are not inconsistent with a feminist teaching of science—“Okay to take risks,” “no wrong answers,” or “team-work”—none critique science and none are overtly feminist. A pedagogue taking the traditional approach—one that Karen Barad (1995) describes as a boyish, Feynmanesque, “physics is phun” one—could have penned them all. This situation might remind those with a knowledge of social history of the social action movements of the 1960s and the lack of direct attention paid to women’s issues in “the Movement,” which eventually spawned a separate women’s movement (Curthoys 1988).

One wonders whether the reason that “the pipeline leaks” and that women leave physics at each ascending professional level is that there is a cost for being different, for speaking in a Gilligan-type (Gilligan 1982) “different voice” among one’s adult peers in the scientific community (Faludi 1991). The mainstream deems it fine to offer introductory courses with alternative curricula based on different ways of knowing. It becomes progressively less fine for one to display the traditionally devalued way of speaking or knowing as one ascends the educational ladder. (Is the assumption, therefore, that these courses will get women and minorities through some immature phase so that they may subsequently use only orthodox scientific expression and practices?) This is reminiscent of the lack of women and “increasing defeminization” as one ascends in other professions, for example, in literary scholarship wherein “the price of the ticket into the professional ranks is, to use Fetterley’s provocative term, cognitive and discursive ‘immasculation’—learning to think and argue like a man” (Schweickart 1996, 314–15). This also resonates with recounted experiences of aspiring professionals who are nonwhite and who function in a white professional sphere: “We had to hang up our nativeness outside the door and come in and think like white people” (Goldberger 1996, 337).

If a “rising tide lifts all boats” philosophy were valid, explicit consideration of feminist ideas would not be necessary in PER. Yet the feminist

² Bibliographies can be found on this subject, including those by Faye A. Chadwell (Roser 1995, 231) and Laura McCullough (2001a). In addition, see Belenky et al. 1986; Campbell 1992; Philbin et al. 1995; Giese 1996; Bauer and Shea 1999; Hodari 1999; Kimura 2000.

literature suggests otherwise. There are different male/female responses to some of the techniques and tests at the forefront of college-level physics reform (Laws, Rosborough, and Poodry 1999; McCullough 2001a, 2001b). While results remain preliminary, tests rewritten to feature female-friendly situations have been found to improve women's scores in greater proportion than men's (McCullough 2001b). Certain questions about rockets and cannonballs may give male students a gender-related advantage (McCullough and Meltzer 2001). Claude M. Steele (1997) notes that the "stereotype threat" implied in merely being asked to put one's race down on a questionnaire before taking a test depresses minority performance, yet it seems slightly to enhance majority performance, especially in mathematics. Elaine Seymour's and Nancy Hewitt's extensive work on why women and men switch out of science, mathematics, or engineering (SME) majors finds a wealth of qualitatively different responses to the same classroom stimuli, depending on the race and gender of the respondent (Seymour 1995; Seymour and Hewitt 1997). Clearly, there is an as yet unrealized opportunity for feminist ideas to join with the dynamic PER movement and infuse some vital knowledge that is rooted in feminist research.

The mainstream has recognized the fact that segregated mentoring or learning environments seem to work in physics, as shown, for example, in the stunning successes of historically black (two-thirds of all African-American physics bachelor's degrees in 1999) and women's colleges (Mulvey and Nicholson 2001). Gender-friendly niches have sprung up: special programs for minorities at major universities and labs, networking activities for graduate students on campus, and women's networking sessions at major meetings. Yet at well-known universities that have some of these outreach programs in place, it is not unusual to also see an entire semester's departmental physics colloquium schedule without a single woman speaker. A well-informed article on the subject of diversity programs at various universities recently published in *Science* magazine in a section titled "Trends in Undergraduate Education" (González 2001) describes strategies, including early involvement in research and mentoring, that allow African-American students to surmount barriers (Rey 2001). Yet, in an adjacent article on the subject of early involvement in research, it is not mentioned that this has proven good for women and minorities (Mervis 2001). In another nearby article, on the newly perceived importance of mentoring at the undergraduate through postdoctoral levels, there is no mention of issues relating to a student's race, gender, or class. The very existence of such articles is heartening. Yet the lack of these natural connections being made between articles on "regular" and "spe-

cial" types of students shows that reforms, while consistent with feminism, have not been fully integrated into the mainstream.

Culture clash, culture lag

In the science wars, scholars who hope for acceptance in both feminist and science communities are sometimes caught in a no (wo)man's land.³ People are rarely able to engage in both scientific and feminist research (Auchincloss 1996). When they do, their energies may be placed in areas that are outmoded from the point of view of mainstream feminist scholars yet radical from the point of view of mainstream scientists. Scott Gilbert (personal communication, 2001) reports that his gender-study group's influential critique of cell biology (the thrust of which is that feminist critiques are scientifically important to a field that is marred by gendered metaphors of fertilization and other processes) went unnoticed and unembraced by mainstream feminist scholars until mainstream antifeminist scholars Paul Gross and Norman Levitt targeted it (Gross and Levitt 1994).⁴ A recent rejection letter for funding to bring a feminism/science performer to the Swarthmore campus reads: "The members of the Committee felt that B is somewhat dated as a feminist performer." The "dated" performance revolved around women scientists who were lost to history and of whom a traditionally educated physicist still learns nothing.

The sluggishness with which natural scientists have set about to reposition women in their history and praxis stands in remarkable contrast to the situation in the humanities. Biographical collections of women scientists published before the 1980s are rare; they have appeared in good numbers only in recent years.⁵ Roeser (1988) studied the paucity of feminist papers at scientific meetings (e.g., the American Association for the Advancement of Science [AAAS]) when compared with the number at a major meeting of humanists (Modern Language Association [MLA]). She has also described the waves of feminist science curricular reform, and she

³ The term *science wars* refers to a recent (within a dozen years) and acrimonious conflict that has arisen between some scientists and some in the field of science studies. The issues center on the validity of critiques of traditional theories of scientific knowledge—whether they are well founded and whether they are injurious to progress in science.

⁴ The paper in question is Beldeco's 1998. It has since been updated as Gilbert 2000.

⁵ See, e.g., Rositter 1995; Shearer and Shearer 1996; Reynolds 1999; Ambrose et al. 2000; Ogilvie 2000; Sullivan 2001.

finds that among a set of prominent national efforts, articulated goals are either at stages 1, 2, or 3 out of six sequential stages (1998).⁶

If first-wave historical feminism is the "liberal feminism" of the 1960s and 1970s (Barton 1998), which corresponds to the "liberal critique" (Keller 1987) of science that addresses skewed employment yet makes no claim that science would change with more women, then mainstream physics is still awash in the first wave. This wave might be said to have crested with the founding, in 1972, of the Committee on the Status of Women in Physics (CSWP) of the American Physical Society (APS), the world's largest professional organization of physicists. The liberal critique found its voice with papers such as Vera Kistiakowsky's (1981) "Women in Physics: Unnecessary, Injurious, and Out of Place?" As the years have passed, the slow progress of women and minorities into the professional sphere has remained a matter of concern (Vetter 1988; Fehrs and Czujko 1992). Currently the mainstream, as represented by organizations like the AIP (American Institute of Physics), the NSF (National Science Foundation), and major universities, is enthusiastically behind the idea that more women are needed at all levels. In the words of Howard Georgi, there is "unconscious discrimination" because physics selects for "assertiveness and single mindedness" (2000). Says Georgi, these qualities are stand-ins for what we really want as a community of physicists, and they map well into a set of men (a feminist might correct that to people who "do male gender" or with "male gender ideologies") who are able to do good physics.

Arguments for including women tend to be based on equity concerns, or the concern that people with good minds should not be turned away when they might be utilized in the service of physics-as-is. Arguments tend not, for example, to be based on feminist empiricist claims that only a diversely gendered group can produce unbiased results or that the problems chosen and methods used must be divested of an existing Western or androcentric bias (Harding 1987). Interestingly, cautionary words along these lines have been articulated by eminent physicists, as in: "It may be easier to adapt oneself to the quantum-theoretical concept of reality when one has not gone through the naive materialistic way of thinking that still prevailed in Europe in the first decades of this century"

⁶ The six Rosser phases of curricular reform might be described as (1) absence of women not noted, (2) absence of women noted, (3) barriers to women's participation identified, (4) inclusion of historical women scientists, (5) inclusion of women and feminist perspectives in the doing of science, and (6) reconstruction and redefinition of science to be unreservedly inclusive

(Heisenberg 1958, 202). Yet mainstream physicists are deeply troubled by feminist epistemologies of science such as those of Helen Longino or Donna Haraway that claim that a community is required in order to interpret and ratify the most fundamental acts of observation and understanding (Haraway 2001; Longino 2001). That a broader community could be generative of more good ideas is not troubling to a physicist. That it could be more objective is. As Evelyn Fox Keller notes, "a first step . . . in extending the feminist critique to the foundations of scientific thought is to reconceptualize objectivity" (1987, 238).

Such a reconceptualization is a move that mainstream physicists are loath to make. Priscilla Auchincloss recently wrote a reasoned and moderate article on physics and feminism in an APS newsletter, suggesting that "feminist studies may hold a key to the success of efforts to attract and retain women . . . create gender equitable environments . . . and to reform physics education" (1998, 15). The responses, in the form of letters to the editor, were not heartening, with one respondent titling his reply "Must We Atone for Sins of the Past?" In calling her use of the word *heresy* "overwrought," the respondent himself spins an emotionally overwrought defense of physics-as-is. Of five replies, only one respondent (male) demonstrated a working knowledge of women's studies and lamented the inability of the monumental present culture to "stop defining women as 'other'" (*APS News Online* 1998).

The anthropological concept of "culture lag" has been applied by Henry Etzkowitz, Carol Kemelgor, and Brian Uzzi (2000) to an antiquated vision that scientists hold of their own sociopolitical structure. Physics certainly lags behind most other sciences in providing a culture that proves desirable to women practitioners. The problem is exacerbated by the real culture of physics hiding behind an antiquated mask. For example, the stereotype of the lone investigator obscures the fact that scientists are members of a community (Harding 1987). Members constantly trade their stocks of "social capital" (Etzkowitz, Kemelgor, and Uzzi 2000). The "lone investigator" myth impedes recruitment and retention of women in many ways; ultimately, a successful woman must weave herself into a social network in order to establish collaborators and win support.

As a second example of a culture lag in physics, consider the widely held view that physics is the most "fundamental" of natural sciences. Barbara Whitten (1996) notes that physicists equate fundamentality with eliteness. She cites evidence that physicists take the existence of a hierarchy among subfields for granted, with work that is most fundamental (i.e., elite) associated with work that probes the tiniest, most elementary con-

stituents of matter. In this hierarchy, it is no surprise that subfields such as biophysics and physics education are at the bottom (Whitten 1996).⁷ (These subfields are interdisciplinary, accessible to more than a few experts, and contain a societal component. Not coincidentally, they attract a better-than-average population of women practitioners.) Whitten argues that a "ladder" of fields is an extremely inadequate picture of science" (1996, 10) and much better is "a web of interconnecting fields, each with its own emergent properties [and] fundamental concepts" (11). This is a more modern and accurate view of physics. It is also more feminist if one agrees with Hillary Rose's comments that science as a top-down industry is not a fertile field for feminist reimaginings. Rose opines that "physics (is) at once the most arcane and the most deeply implicated in the capitalist system of domination. At the same time, the physical sciences more or less successfully exclude any more than small numbers of women" (1987, 272).

In summary, as if the true culture of physics were not "chilly" enough for women and minorities (Franz 1995), women are additionally encumbered by certain obsolete, inaccurate perceptions of the sociology of physics.⁸ I have given just a couple of examples of such perceptions. These perceptions are, unfortunately, rooted in the imaginations of physical scientists themselves. In a recent study of imagery in standard geology textbooks, not only did photos and diagrams of people usually depict males but, while photos depicted males 72 percent of the time, diagrams/drawings did so 95 percent of the time (Phillips and Hausbeck 2001).

What next?

As meager as the feminist content sometimes is, and as divorced as it is from its identity as feminist content, I believe that feminism *has* made a significant impact on the field of physics. The specific nature of modern curricular reform and the establishment of various niche activities for women provide two bodies of evidence that support this claim. The "production of people" has traditionally been viewed as a female vo-

⁷ Londa Schiebinger has also made this point, as well as cited Sandra Harding on it. See Schiebinger 1999, chap. 9.

⁸ The Franz paper documents an APS/NSF-sponsored project led by Franz, Mildred Dresselhaus, and Bunny Clark to do a national survey of graduate and undergraduate students on issues of departmental climate. The results of this study have been a catalyst for positive change in faculty recruitment, creation of safe spaces, opportunities to network with other women on campus, etc.

cation (Rose 1987). While it is vital not to reinforce gender stereotypes nor to limit the scope of feminist reform by suggesting that it adhere to outmoded traditions, it would be negligent to fail to observe here that a commitment to nurturance is apparent in various key mission statements. The Committee on the Status of Women in Physics (2002) attempts "to address the encouragement and career development of women physicists"; the Society of Women Engineers (2002) hopes to provide "positive stimulation for the achievement of full potential" and "nurturance of leadership skills."

Many questions remain about the future of feminism and physics. Inspired pedagogues have already created a role for different voices and ways of knowing in introductory and niche or interdisciplinary classes.⁹ One wonders, however, whether there will eventually be the same role for feminist pedagogy in every physics, chemistry, or math course. Also, while there have been a number of workers who have contributed substantially to a feminist epistemology of physics, among them Priscilla Auchincloss, Karen Barad, Evelyn Fox Keller, Sandra Harding, N. Katherine Hayles, Kristina Rollin, Sharon Traweck, and Barbara Whitten, one wonders whether feminism will someday have the epistemological relevance to mainstream physics that it does to other fields where the human connection is more manifest.

In an attempt to predict the answers to such questions, one might note that the methodologies and goals of feminism and physics actually have many similarities. Consider for example these precepts of feminism:

Thought and action must be unified.

Working communities are antielitist.

These have also been central principles of physics from its inception (Keller 1985): "mens et manus" (mind and hands) is the motto of MIT. (Keller, though, notes the following problems with implementation of the latter principle: ironically, the overthrow of ancient authority did not include the overthrow of male authority; and the social component of the revolution was subverted, so that social elites controlled the British Royal Society.) Both are principles of Baconian science.

On the other hand, consider the principles of feminist science, as they have been defined by Ruth Bleier (1986) and as I have listed them in the chart on the next page. In the chart, I assess whether these form part of

⁹ See, e.g., Barad 1995; Rosser 1995; Schwarz 1996; Barton 1998; Shulman 2001, Wessell, Honrado, and Bautista 2001.

the orthodox practice of physics as displayed in mainstream meetings or in our mainstream literature.

Feminist Scientists	Physicists
a) Acknowledge their values and beliefs	No
b) Explore how these affect their perspectives	No
c) Are explicit and honest about assumptions and methods	Yes
d) Are responsible in language	Yes (math) No (metaphors)
e) Eliminate research leading to exploitation of nature	No
f) Aim for diversity among participants	Yes
g) Recognize the complexity of nature	Yes
h) Resist single-cause explanations stripped of social context	No

The poor fit between these principles and physical practice suggests that feminist principles may be so at odds with physics that to be true to both fields is to profoundly distort both of them. In the words of Harding, "neither women's activities nor gender relations . . . can be added to . . . theoretical discourses without distorting the discourses and our subject matters" (1987, 283). As I have mentioned earlier, there is an intense resistance to feminist reimaginings of orthodox physics. Steve Gensemer, an atomic physicist who is interested in issues of women in physics, has written: "Inventing a feminist version of quantum mechanics seems an incredible waste of time after all. What is needed is a way of understanding the cultural, social, gender, etc. issues in physics without throwing away our hard-won knowledge" (1998). This is a telling metaphor, one of knowledge not constructed but won, as in a battle. "Combat physics" is how women at an international conference this year described a familiar mode of scholarly interaction (Feder 2002).

If feminism is unable to further have an impact on the field of physics and if physical science continues to attract scant attention from mainstream feminists, both fields will be the poorer for it. Women's studies and feminist theory have insights to offer physics—if physicists would only permit it! Consider these five closing ideas as launching points for future thought and discussion:

First, the question "Is physics-as-is androgynous, value free, and the best physics that humanity can produce?" is not a foolish question meriting an automatic, affirmative answer. (Harding has posed a number of interesting, related questions [1991, chap. 4].) To begin with, one cannot claim that physics is "gender neutral," since feminist science studies teaches one to define endeavors from the lives of the practitioners. The very fact

that women's participation in physics has lagged behind other academic disciplines and other sciences demonstrates the point. To assert otherwise is to envision a "physics" that is part of a shadow world of idealized forms, not a discipline in which real people engage. Furthermore, the physics literature is not value free. Bias is rarely as blatant as, for example, in a case history of the Hubble constant that Sharon Begley reported (2000), where one's academic affiliation determined which of two values of this important constant one espoused. Yet, when ideas are presented, no matter how neutral the language, they can make the oblique point that they are ascendant and certain others are in eclipse. When a paper begins, as so many do, "Recently, there has been much interest in X," the value of community stands out. As Bonnie Shulman (2001, 416) reminds us, based on her wealth of evidence drawn from the history of mathematics, we must teach students to "*expect* a standpoint in any scientific statement."

Second, one cannot understand the "Why so few . . . ?" problem in physics without knowing some basic women's studies, black studies, Latino studies, and so forth. For example, the gender division of labor between public and private spheres is mirrored in the work choices in the public sphere. This is integrally related to the low numbers of women in physics and, as mentioned earlier in the context of Whitten's work, the existence of "woman-rich" (e.g., biophysics) and "woman-poor" (e.g., plasma physics) subfields.

Third, physicists themselves do not speak in a single voice but use a "different voice" and different manners of thinking as the occasion warrants. There has always been a dialectic between what Karl Sterne calls "scientific and poetic knowledge" (1965). The best physicists have always kept the two in balance and known when to apply a greater proportion of one or the other. There are numerous examples of eminent physicists who have transported both terminology and substance from one discipline to another—for example, from religion to physics, or physics to society.¹⁰ For various reasons having to do with communication and conceptualization, physicists sometimes produce creative images or narratives, much in the manner that Cindy Schwarz (1996) encourages students to do in her courses on particle physics at Vassar College. Below is an example drawn from the study of positrons and electrons. These elementary particles are "opposites" in a very rigorous sense of the word; moreover, when they meet, they annihilate in a flash of energy. Figure 1 depicts an electron and positron as belonging to the "opposite" (1) sexes. It was

¹⁰ See, e.g., Wertheim 1995a, 1995b, 1995c, 1995d; Beller 1998; McGrail 1998; Barbour 2000.

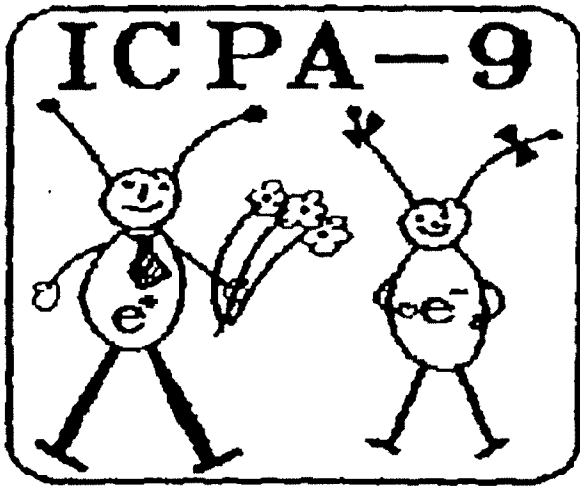


Figure 1 Electron meets positron

chosen by a leader in the field, Zsolt Kajcsos, who included it on the title page of a major conference proceedings volume that he edited (1992). Kajcsos told me that it was his little daughter who drew them—"based on the evening tales I told her." While this example may seem to reify traditionally held gender relations, it holds elements of critique available to adult viewers. (The truly "opposite" character of particle and antiparticle renders the notion of "opposite sexes" ludicrous by comparison!) This image is important in another way: it chronicles a small girl's writing herself—writing an unmistakably female image—into the story and into the physics.

Fourth, feminist epistemology of science is not monolithic. There are many such theories of scientific knowledge. Mainstream physics might be strongly persuaded by one of them, perhaps by that of Kristina Rolin (1999), a philosopher of science who has recently argued that the activities that constitute discovery and/or the formulation and justification of cognitive goals in physics are clearly embedded in the discoverer's gender identity. Rolin does not argue for relative truth of results of physical calculations or experiments, and she asserts that the overarching cognitive value in physics is empirical predictability—ideas embraced by mainstream physics. Tools of gender analysis are available to the physicist who would like to explore, for himself or herself, the distinctly gender-related elements of the discipline (Schiebinger 1999, 186–90).

Finally, ever since the inception of quantum mechanics a century ago

physicists have recognized that there is a problem with the concept of "quantum reality." In the mainstream literature that addresses "quantum weirdness," some feminist contributions could be profitably included. Keller's 1985 essay "Cognitive Repression in Contemporary Physics" (chap. 7) contains a clear description of what must be dropped from the theory: an obsolete notion of objectifiability. Barad's theory of agential realism makes it clear that one may only talk of an "intra-action" between the knower and the known, that without the collaboration of the agent of observation, the idea of "objective properties" is not a sensible one (Barad 1996, 2000). A recent contribution in the orthodox literature, "relational quantum mechanics" (Rovelli 1996), is not an overtly feminist theory, yet one might argue that it is nevertheless a Rosser phase 6 theory (Rosser 1998).¹¹ It aspires to provide postulates from which quantum theory can be derived, or "reconstructed" in the words of Rovelli. Furthermore, and in agreement with the premises of Barad's work, Rovelli writes, "Physics is the theory of the relative information that systems have about each other. . . . I reject any fundamental distinctions as system/observer . . . physical system/consciousness" (1996, 1647). Relational quantum mechanics (Rovelli 1996; Smolin 2001) has obvious links to the "strong objectivity" of Harding (1991, chap. 6). Interestingly, while strong objectivity is so named because the traditional, nonrelative type of scientific objectivity is viewed to be weaker, just the opposite valuation is placed by Rovelli: "I want to *weaken* all physical statements that we make: not 'the spin is up,' but 'we have information that the spin is up'—which leaves the possibility open to the fact that somebody other has different information" (1996, 1646; my emphasis).

Should we be surprised that, so far, feminism has done little to accelerate the minds and hearts of physicists? Should we expect that women will continue to join the physics community and that as the population rises toward some critical mass, a feminist physicist consciousness will continue to mature? Although I have no answer, I hope that the reader has found some value in a journey through various possibilities and conjectures. In the physics community, as in the feminist community, the posing of interesting problems and the production of conjectures—even when eventually overturned—are validated activities.

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¹¹ See n. 6 above.

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Sexual Natures: How Feminism Changed Evolutionary Biology

Parental investment theory (Trivers 1972) argues that the supposed natures of males and females originated in most sexual species with ancient selection pressures that favored more parental care by mothers than by fathers, which in turn favored discriminating, passive females and competitive, profligate, and aggressive males.¹ The logic is based on the fact that females usually have more to lose than males through poor reproductive decisions, so that selection favored careful, choosy females. Parental investment theory has been a flash point of controversy within and outside evolutionary biology. Beginning most prominently with Edward O. Wilson's (1975) statements about the evolutionary source of typical differences between women and men, it seemed to many a con-

I thank Londa Schiebinger for inviting me to participate in the conference that resulted in this article and for her help in clarifying my thoughts throughout the process of review and publication. I am grateful to Steve Hubbell for his advice, particularly that I read his 1987 paper cowritten with Leslie K. Johnson. I also thank my students, particularly Beth Tyler, for their critical comments on this work.

¹ I use the term *sex* in reference to males and females in sexual species, because to me the term *sex* is silent about the mechanisms of sexual or gendered behavior, physiology, or morphology. "Gendered" or sexual behavior in Eastern bluebirds, the species that I have studied for twenty-five years, can be due to intrinsic differences between individuals with big or small gametes or as easily to environmentally induced factors that affect developmental expression of behavior. Even more likely is the possibility that sex-typical behavior is due to the interaction of intrinsic and environmentally induced factors. Thus, around 1988, I reverted to using the term *sex* in spoken and written formats (including grant proposals) as the inclusive term for Eastern bluebirds (e.g., see Gowaty and Plummer 1998). By analogy, I think the term must work for humans too, whether the *sexed* trait is determined strictly by environmental factors, development, genes, or the interactions among the three. Readers might keep in mind that *sex* in reference to males and females to many biologists implies no more than the relative size of their gametes. Recently, my views about the word *gender* just expressed are in the process of change. In an unpublished manuscript, Joan Roughgarden puts forth the most interesting discussion of gender in nonhuman animals that I have seen. I have taken her discussion of gender in nonhumans quite seriously and suspect that in the near future I will again be using the term in discussions of nonhumans.

venient, untested "just-so story" that buttressed status quo notions about sex roles that were sometimes used to confine women to their "natural" roles as mothers and subordinates to men.²

The fact that in 99 percent or more of sexual species females have bigger gametes (sex cells) than males is consistent with parental investment theory. This fact has allowed it to achieve axiomatic status in sociobiology and evolutionary psychology during the last thirty years. For many, it has enormous intuitive appeal. It is a ubiquitous feature in elementary animal behavior, behavioral ecology, and evolutionary ecology texts. Yet investigators have rarely tested how parental investment is associated with sex roles in species with "typical" mother-biased offspring care patterns.³

Feminist critics have repeatedly questioned parental investment theory, noting that often observation fails to match its predictions. Females are often aggressive and enthusiastic about sex even in species with female-biased parental investment. The most successful of these critics, anthropologist and evolutionary biologist Sarah Hrdy (1986, 1997, 1999b) has made inroads within primatology. Evolutionary psychologists, however, have all but ignored her challenges to the basic story of how patterns of parental investment affect sex roles in humans.⁴

Thus, my answer to the question that motivated this cluster of articles, "Has feminism changed science?" is "yes, but we still have a way to go." I think this particularly in relation to this flash point about fundamental sexual natures. Interest in "fundamental natures" remains high, but tests in "typical" species remain uncommon. The tension between those for whom the answer is obvious and those of us who think the question is mostly unanswered signals a tension that may stimulate new work. What most agree feminism has done is facilitate the entrance into and retention of women in science (Schiebinger 1999). And although some feminist discourse seems antagonistic to science, many of my colleagues pay attention to feminist-inspired scientific ideas—whether they come from women or men and even when investigators explicitly identify their source from within late twentieth-century, early twenty-first-century, Western feminism. I suspect tolerance is common. By "tolerance" I mean that ideas that come out of feminism are taken seriously as long as they are discussed in terms of their testability, which is the hallmark of all ideas

² I think of "just-so stories" as hypotheses. When hypotheses are testable, they are the best guides to research (Gowaty 2001).

³ It has been tested in species with male-biased parental investment.

⁴ See the 1999 preface to the revised *The Woman That Never Evolved*, first published in 1981.

tolerated by scientists. Feminist consciousness is not limited to women, so that the consciousness of both men and women to feminist issues has increased and is changing evolutionary biology. Feminist-inspired hypotheses are becoming easier to publish. Changing science, however, depends not so much on critical discourse but ultimately on strong, inferential tests of the predictions of hypotheses. What changes normal science, as often as not, is the accumulation of data inconsistent with current dominant hypotheses.

What I hope to show here is that the most efficient route to changing an entrenched scientific idea lies in carefully designed, well-controlled, empirical tests. I hope this challenge is an invitation to women and men of feminist consciousness to enter science. It is likely that the strongest feminist contribution will continue to be the expansion of opportunities within our discipline for women. But, whether more women enter science or not, opportunities to test feminist-inspired hypotheses increase all the time. And men as well as women are testing them using strongly inferential, empirical tests.

To illustrate some of these opportunities, I describe both parental investment theory and alternative theories that also explain "sex roles." I then present naturalistic observations inconsistent with parental investment theory and experiments that revealed that in some species the coy nature of females results from males' manipulations of females' reproductive decisions. Finally, I describe tests of parental investment theory that control for existing alternative explanations. I have also been as explicit as my personal knowledge allows about the role of feminist consciousness in the development of sex role science. I conclude that many more tests of the "basic" natures of females and males in a variety of organisms are necessary. *The main points are that the "basic natures" of males and females remain to be described in the vast majority of species; interesting alternative hypotheses exist to explain sex roles; and these alternative hypotheses offer an empirical challenge to those interested in understanding the causes and consequences of sexual and reproductive behavior.*

A brief intellectual history

Parental investment theory was inspired by a classic study by geneticist A. J. Bateman (1948), who was stimulated by Charles Darwin's (1871) ideas about natural and sexual selection. Selection occurs when some individuals survive and reproduce better than others due to heritable trait variation among individuals within populations, and selection is likely to be strongest when the variance in reproductive success among individuals

is large. Sexual selection is therefore an aspect of natural selection having to do specifically with variation in reproductive success among members of the same sex and species. When the among-male variance in reproductive success is large and due to heritable trait variation, selection is strong and favors the persistence of traits in the descendants of those that reproduce the most. Darwin emphasized two mechanisms of sexual selection that can create variation in reproductive success among males. Male-male combat is a mechanism of sexual selection whenever it results in higher mating or reproductive success for winners than for losers. Female mate choice also results in sexual selection among males whenever females prefer some males to others and reproduce with them more often than with those they do not prefer.

Bateman's experiment was the first to report the widespread phenomenon (Clutton-Brock 1984) of greater variance in mating success (number of mates or copulation partners) among males than among females, which is what one would expect if male-male competition and female choice affected the differential success of males. Bateman put flies of each sex of *Drosophila melanogaster* into mating vials. Bateman inferred each male's reproductive success using paternally inherited phenotypic markers in the progeny. If the sex ratio is equal and if some males mate with more females than other males, some males will be unable or less likely to mate. The resulting among-male variance in mating success will be large. If all the females are likely to mate, and most mate only once, the variance in mating success among females is likely to be smaller than among males.

Bateman observed greater variance among males (fig. 1A) than among females (fig. 1B). Most males had no mates or one or two, but nearly 17 percent of males had three or four mates. Variance among females (fig. 1B) in mating success was lower. A tiny percentage of females had no mates, but the vast majority had one or two mates, and another tiny percentage had three. The number of mates any female had was similar to the number any other female had. Among-sex variances are important, because whenever large differences in fitness measures (such as mating success) among individuals of one sex occur, strong selection will occur if these differences are associated with traits in the winners. Thus, these differences suggest that there is strong selection among males based on mating success and weaker selection among females *based on mating success*. Beginning with this study many assumed—incorrectly, as we now know (Rice 1996)—that selection was invariably stronger among males than among females. Bateman concluded that selection among males was driven by male-male combat and female choice of males, and he concluded that his observations were consistent with Darwin's mechanisms of sexual

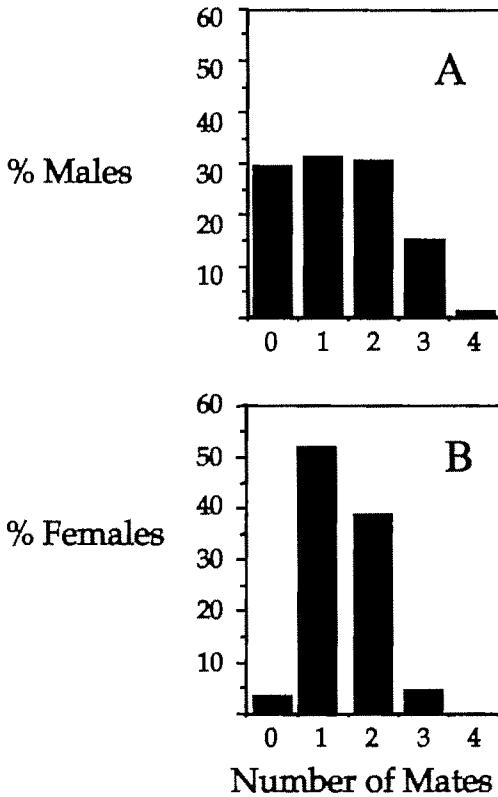


Figure 1 Data on mating success variance among males (A) and females (B) graphed from Bateman 1948.

selection among males, namely “undiscriminating eagerness in males and discriminating passivity in females” (Bateman 1948, 367).

In his important book *Adaptation and Natural Selection*, George C. Williams (1966) extended Bateman’s conclusion to a general argument explaining why females were generally coy about mating. He said females make greater physiological sacrifices that make pregnancy a burden, which would explain why females are coy and discriminating in comparison to eager and profligate males. While even undergraduates take issue with this argument, Williams’s ideas about mammals were easily extrapolated to other organisms, even invertebrates such as fruit flies in the genus *Drosophila*, because biologists define sex in terms of gamete size. In most, but not all, sexual organisms, females have larger gametes than males. Maternally derived nutritive material usually surrounds eggs, while sperm are most often little more than a packet of haploid chromosomes with a

vibrating tail. Others subsequently argued that there have been many episodes of selection on "sex role" behavior since the origins of anisogamy (different-sized gametes). Despite objections, the argument continues to be a compelling explanation for sex differences.

Robert L. Trivers (1972) further generalized Bateman and Williams. He explained "typical" sex roles of discriminating, "coy" females and indiscriminate, "ardent" males for most organisms based not just on anisogamy but on parental investment. Trivers defined parental investment as "any investment in an individual offspring that increases the offspring's chance of surviving (and thus reproductive success) at the cost of the parents' ability to invest in other offspring" (139). He explained why it was usually males that were so active about sex and competitive, while females were so discriminating, choosy, hesitant, passive, and "coy." He thereby explained why male-male competition and female mate choice seemed to be the two most prevalent mechanisms of sexual selection. Ever since Darwin, the "facts" of choosy females and profligate males have organized studies of social behavior evolution. Ever since Bateman, Williams, and Trivers, parental care patterns have explained indiscriminate, profligate, competitive, and aggressive males and coy, hesitant, passive, highly discriminating females. *The "facts" about males and females have been so intuitively obvious that only a few ever asked if the "facts" were correct.* Parental investment theory was also powerful in its novel predictions. The scientists who tested them (see below) changed the debates about the origins of sex roles and helped open behavioral ecology to a welcome, still-ongoing discussion of "females' perspectives."⁶ Cited thousands of times, Trivers's 1972 work is a citation classic.⁶

⁵ Rosenqvist and Berglund 1992; Ahnesjö et al. 1993; Gowaty 1997a; Parker and Burley 1997.

⁶ Despite challenges to the primacy of the parental investment idea, Trivers's (1972) paper deserves its status as a citation classic. Among the very important things it did was to legitimize the study of female choice, which was hardly taken seriously as a mechanism of reproductive competition even one hundred years after Darwin first published the idea. Today the effect of female mating preferences on traits in males is one of the best-developed areas of research in all of biology. What was doubted in Darwin's day is indeed fact today. Females' preferences for particular phenotypic or underlying genetic traits of males account for the evolution of many of the bizarre and elaborate secondary sexual traits of males, and much of our current understanding of female choice as a force in evolution must be attributed to the stimulating effect of Trivers 1972. This paper also almost single-handedly motivated the many studies of extra-pair paternity in socially monogamous creatures that have shown overwhelmingly that social monogamy should not imply "faithfulness." Trivers's ideas galvanized a generation of empiricists, and his positive influence on the biological study of social behavior cannot be overemphasized.

Crucial observations failed to match predictions

Parental investment theory's important sex role prediction was that aggressive, competitive females and restrained males occur in species in which the sexes are "role reversed" (i.e., father-biased parental care). This prediction made sea horses and pipefish model organisms (Vincent 1992). Using these species, biologists familiar with their life histories were able to critically test the theory's predictions. Male sea horses and pipefish brood offspring in a ventral pouch, so that males "get pregnant." As predicted, males are choosy, and access to males limits female reproductive success (Berglund and Rosenqvist 1990; Rosenqvist 1990). A bit more surprising was that females were choosy, too (Vincent 1994; Vincent, Ahnesjö, and Berglund 1994), even though they do not care for offspring. The great surprise, however, was that even though males invested most in offspring, they still seemed to compete more than females did for access to members of the opposite sex. It seems significant to the context of this article that Amanda Vincent and many other investigators interested in species with reversed sex roles are women, some with a self-acknowledged commitment to feminism (Bolker et al. 1997).

Perhaps more troublesome than the failures of predictions in "role-reversed" species were the observations of extraordinarily aggressive, anything-but-passive-coy-disinterested-discreet female primates (Hrdy 1986, 1997, 1999a, 1999b). Female chimpanzees and langurs, among others, enthusiastically solicit sex, and some females have been observed to mate with up to eight different males within an hour's time! Hardly the behavior predicted for a species with a long gestation similar to humans, a costly period of lactation (up to four years, as in some human populations), and one of the longest periods of offspring dependence for any nonhuman animal. As with so much else in the feminist-inspired evolutionary biology of the last twenty-five years, Sarah Hrdy led the way. She argued that the promiscuous sexuality of female chimpanzees and human langurs was a result of selection favoring the confusion of paternity as a female resistance mechanism to sexually selected infanticide by adult males.

Chimpanzees and langurs, of course, are not the only primate species with ardent females. Other primates in which females solicit sex from sometimes seemingly bored males are difficult to explain based on variation in parental investment patterns. Note, too, that these observations about common chimpanzees and other old-world primates were available long before anthropologists, including feminist Amy Parish (1994; Manson et al. 1997; Parish and De Waal 2000), brought the extraordinarily enthusiastic sexuality of female bonobos to our collective attention.

One explanation for the apparent indifference of some male primates to the sexual solicitations of females is that male coyness is due to sperm limitation. Servicing females results in declines in sperm reserves. When males failed to immediately copulate with a soliciting female, researchers did not imagine that in these typical primate species the males might simply be discriminating, passive, or simply uninterested in sex with the soliciting female. Rather, they hypothesized that the costliness of producing sperm makes males reluctant to mate. This was likely to be especially so in polygynous species, where males have more than one social mate and supposedly also more than one sexual partner. However, there are almost no studies of sperm limitation (Pitnick 1993) and its power to affect male nature, particularly in primates. *The point here is that despite observations inconsistent with intuitive claims, passive females and eager males remain the normative expectation.*

Alternative theories for sex roles

Besides the notable failures of nature to conform to randy-aggressive-ready males and fussy-passive-unenthusiastic females, alternative theories explaining Bateman's patterns exist. The earliest of the alternatives was a null model, whose author argued that the differences Bateman observed could have arisen by chance (Sutherland 1985). William J. Sutherland showed that random factors, rather than or as well as male competitiveness and female discrimination, could account for the differences in mating success variances between the sexes. He considered how "handling time" affected the within-sex variances in mating success. Handling time was the time it took to copulate plus the time between the end of copulation with one partner and time to the onset of receptivity to remating, which might include parental care or regaining nutritional readiness to reproduce again. He calculated distributions of mating frequencies for different values of handling time and found that male and female differences in handling time could account for the variance differences Bateman reported. Thus, an alternative hypothesis challenged Bateman's conclusions, because sex-associated "handling times" as easily result in sex-associated mating success variance that Bateman previously had attributed to nonrandom mating because females were choosy. *What was most important about Sutherland's model was that it showed that sex differences in variance in the number of mates should not count as evidence for indiscriminate males and coy, choosy females.* It is remarkable that hundreds of attempts to experimentally describe sex roles in "typical" species did not follow after the publication of Sutherland's paper.

Stephen P. Hubbell and Leslie K. Johnson (1987), a theoretical ecologist and an animal behaviorist, both self-described feminists (S. P. Hubbell, personal communication, November 1997), were inspired by Sutherland (1985) and their own earlier work on role-reversed beetles (Johnson and Hubbell 1984). They used mathematical models to explore when random environmental variation would favor or select against choosiness versus indiscriminate mating. Their model was a hypothesis explaining differences in reproductive success variances that depended on time-varying parameters, and it was sex neutral, applying to individuals regardless of their sex. The model examined differences in the mating success of strategists that mated with anyone that came along (indiscriminate mating strategy) or "held out" for a higher quality mate (choosy strategy). The time-sensitive parameters of the model included the strategists' probability of survival, the amount of time they had spent in "handling" the previous mating, their encounter rate per unit time with potentially mating individuals, and the probability that they would encounter individuals that would confer on them relatively high reproductive success. Their results showed that selection favors choosy strategists when the strategist's survival rate is high, when encounter rates are high, when the amount of time strategists had spent "handling" the previous mating was long, and when their relative reproductive success with any two potential mates was different. Figure 2 shows the range of relative reproductive success conferred by potential mates over which selection will favor indiscriminate and choosy strategists. The lines represent the boundaries where the fitness payout for choosy versus indiscriminate strategists is equal under each set of parameter values. As encounter probabilities, ap , go from very high (fig. 2A) to relatively low (fig. 2B) to very low (fig. 2C), the fitness payout for indiscriminate strategists increases. *The Hubbell and Johnson model suggested that mating strategies need not be due to sex differences fixed by ancient selection pressures. It showed when selection will be against choosy females and indiscriminate males, even in typical species with higher female than male parental investment. Most important, to conclude that it is indeed the case, empiricists must now account for the theoretically robust effects of real-time variation in temporal parameters of individuals' lives.*

Their alternative hypothesis implicitly suggested that selection should work so that individuals respond flexibly, induced by real-time environmental contingencies to be choosy or indiscriminate. Evolved sexual natures may be flexible, adjustable to changing environmental circumstances. This idea would seem to be particularly applicable to those species that

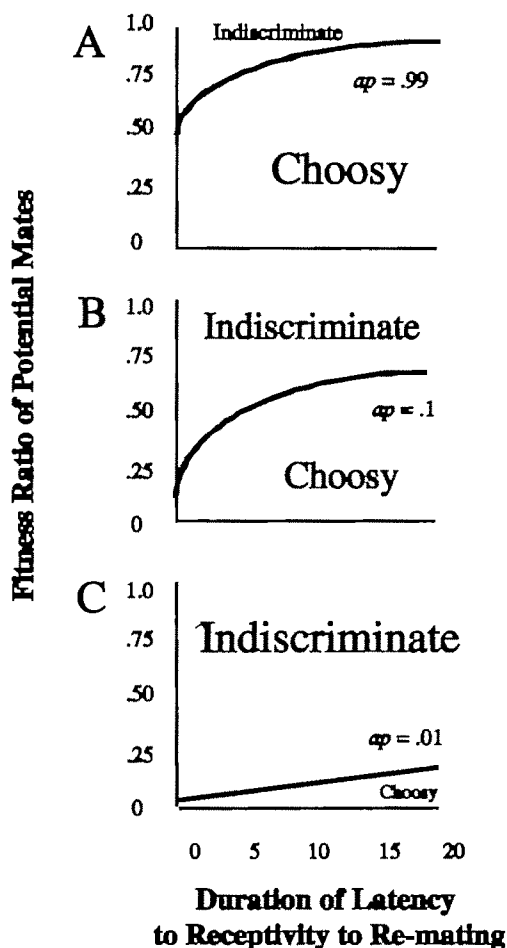


Figure 2 This figure, modified from Hubbell and Johnson 1987, shows how the strategies "choosy" and "indiscriminate" will vary as functions of fitness differences of potential mates (fitness ratios) and the duration of latency to receptivity to re-mating when strategists' encounters with potential mates, ap , vary. The line represents the boundary where the fitness payout for choosy versus indiscriminate strategists is equal.

live in socially varying environments or those in which the sexual landscape might change over the course of a single life.

There are no direct tests of the Hubbell and Johnson idea. In fact, since its publication, others have cited it forty times, a relatively small number for a prominently published scientific paper. It would be particularly interesting to test their predictions, because it should be easy to experimentally manipulate variables associated with the model's param-

ters. If the model is correct, as males encounter more females the frequency of choosy males should increase, too—even in species in which males contribute no parental care and have a very short latency to remating. As females encounter fewer and fewer males, they should more frequently express the indiscriminate strategy and mate with males as they encounter them. *My point about Hubbell and Johnson (1987) is that credible, sex-neutral alternatives to parental investment theory exist, and those interested in social behavior must eliminate these alternatives before concluding that parental investment patterns alone account for selection on sex roles.*

Other studies focused on predicting which sex would compete over the other. A derivative prediction of parental investment theory is that if one sex is choosy, the other should compete over access to the choosy sex. A line of research began when Vincent discovered that in sea horses, with paternal “pregnancies” and no postgametic maternal care, both males and females were choosy, and males but not females were competitive. She explained that female choosiness could be due to females’ high egg investment and male choosiness due to males’ investment during their “pregnancies.” In an attempt to explain the harder-to-rationalize male competitiveness, Tim Clutton-Brock and Amanda Vincent (1991) analyzed the relationship between reproductive rates and the probability of competitiveness. Reproductive rate is a variable analogous to Sutherland’s “handling time” and to Hubbell and Johnson’s “latency to receptivity to remating.” Clutton-Brock and Vincent found that sex differences in reproductive rate predicted which sex was competitive in some, but not all, cases of “role reversal” in fish and bird species. Nevertheless, they concluded that when the sex with the most parental care also had the higher potential reproductive rate (lowest handling time), that sex competed more intensely for mates. They then reasoned that when the adult sex ratio was even, the sex with the slower reproductive rate was not available for mating as often as the sex with the faster reproductive rate. Thus, if the absolute sex ratio is even, but reproductive rates differ, they reasoned that members of the more often available sex must compete for access to members of the less available sex. Sex ratio is a loose correlate of which sex competes over access to mates, but overwhelming empirical support for the organizing effects of sex ratios does not yet exist. Nevertheless, Tim Clutton-Brock and Geoffrey Parker (1992) formalized these arguments and reinserted “intrinsic” variation back into the formula for sex roles by emphasizing that the main contribution to the differences in sex ratio of available mates was variation in parental investment.

Twenty years after Trivers, sex role scientists concluded that females were choosy all the time, males were choosy when they contributed sub-

stantial parental investment, and members of the most abundant sex (usually males) were competitive. Predicting which sex was competitive became the important task. Powerful alternatives remained untested. The studies of Sutherland as well as of Hubbell and Johnson should have stimulated tests of sex roles in species with typical female-biased parental investment. But, even today, almost no systematic studies of variation in sexual restraint versus aggressive profligacy in "typical species" exist. Sex role studies should control for sperm limitation, latency to remating receptivity, survival rates, encounter rates, and variation in the relative fitness effects of mating with alternative potential mates. I know of only two studies that have controlled sperm limitation, latency to remating receptivity, and encounter rates while asking what the effect of parental investment patterns are on sex roles (Gowaty et al. 2002, 2003). *Until experimentalists eliminate or control these and other alternatives, such as the one discussed below, conclusions about inevitable sex role differences determined by parental investment patterns are premature.*

Male manipulation may also explain females' "discriminating passivity"

The most common alternative for restrained female sexuality is that males coerce or manipulate female behavior, sexuality, and reproductive decisions. Barbara Smuts, another scientist with feminist consciousness, said that it is premature to claim that women's relative lack of interest in sexual variety is due to selected nature, when there is remarkable evidence that women are beaten and murdered for suspected or real promiscuity and adultery. If female sexuality is biologically muted by ancient selection pressures, why must men and their families go to extreme lengths to control and contain it (Smuts 1992; Smuts and Smuts 1993)?

The funniest statement of this alternative appears in *Woman: An Intimate Geography* by Natalie Angier (1999), a feminist science writer. She was responding to experiments in which attractive shills approached opposite-sex students and asked them to have sex. None of the women accepted, but 75 percent of the men did.

Let us hold a kaffeeklatch. . . . Women don't want to take a man up on his off-the-quad overture. Fancy that. Women don't want to take a strange and obviously aggressive man back to their dorm room or apartment for a quickie. Could it be that they are in fear of their life rather than uninterested in the pleasure a handsome man might bring them? And could it be that young women just

don't scare men physically the way young men do women? If there were no legitimate fear among the women, surely at least a couple of them would have turned out to be of the "whore" phenotype that supposedly characterizes some women and said yes. Moreover, I wonder how many of the men who said "Count me in!" to their solicitor would have followed through to a bona fide act of intercourse, would not have been a little nervous when push came to shove, if you will, about this forward, lascivious, inappropriately behaving dame . . . ? In other words, were the men for real, or was it bluster? (Angier 1999, 335-36)

Sex role scientists who study nonhumans have seldom taken seriously the idea that males' behavior may create coy females. Because access to females limits male reproductive success, selection should favor males that manipulate or coerce females, unless female resistance increases the costs to males of such behavior.⁷ Smuts brought male coercion into the discussion of sexual selection when she pointed out that variation among males in their abilities to coerce females could lead to mating success variance among males. Until her paper, many biologists found it hard to understand how aggressive interactions between males and females could lead to higher variance among males in mating success. A similar argument works to explain sexual selection among females. If variation among females in their abilities to resist male manipulation exists, females' resistance to male coercion could lead to high fitness variance among females and female sexual selection. I took my inspiration for ideas about female resistance out of my experiences as a Western feminist. I modified the rhetoric of the women's movement into hypotheses with specific, testable predictions (Gowaty 1996a; Gowaty and Buschhaus 1998).

That males manipulate females to be coy recently gained added credibility from studies of Bateman's fly, *Drosophila melanogaster*. Once mated, female *D. melanogaster* are not receptive to mating again for three to four days, a phenomenon known as "remating inhibition." Factors that inhibit remating altogether or increase the period of inhibition increase handling time and resultant choosiness (Johnson and Hubbell 1984) and decrease female reproductive rate (Clutton-Brock and Vincent 1991). So, it would seem interesting to know why remating inhibition occurs and how. It has been known since the 1960s that some component in the inseminate of males was associated with the dramatic change in receptivity of female *D. melanogaster*. Recent studies identified

⁷ Gowaty 1992, 1996a, 1996b, 1997b, 1999; Gowaty and Buschhaus 1998.

ejaculated peptides from the accessory glands of males as the initial cause (Wolfner 1997). Furthermore, exposure to these peptides decreases female life span (Chapman and Partridge 1996; Chapman et al. 1998; Chapman et al. 2000). Readers who have made it this far might recognize the significance of this observation for understanding the origins of female natures. Male chemical "weapons" may account for variation in female *D. melanogaster* latency to receptivity to remating and for some, anyway, of the "discriminating passivity" of females.

Hubbell and Johnson's model suggests how male-induced discriminating passivity of females might occur. A small change in male-induced latency (from one consecutive time unit to another) of females might change an "undiscriminating eagerness" into a "discriminating passivity." Consider figure 2 again. Assume that individual virgin females and males searching or waiting for mates encounter a potential mate during 20 percent of time units. (Remember that the curve is the fitness space among potential mates for a given period of latency to receptivity to remating that yields an equal fitness for strategists that are choosy or indiscriminate. Remember also that a fitness ratio of, say, .2 means that expected fitness with one mate is only 20 percent of what it would be with the other possibility. Similarly, a fitness ratio of .5 means that expected fitness with one mate is 50 percent of what it would be with the other possibility. The difference in fitness among potential mates is greatest when the fitness ratios are the smallest.) Before mating, virgin females and males have equal latency to receptivity to remating of zero, because neither has ever mated before, so this model parameter does not apply to virgins. One can see in figure 2 that over most differences in fitness (vertical axis) conferred on strategists by mating with one or the other possible mate, individuals should be indiscriminate—the fitness space above the line of equal probability of the two strategies, when latency to receptivity to remating is zero. Females and males should be choosy when the fitness ratio is small, indicating very large differences in fitness among potential mates (in this example, from a ratio of zero to .2). This means that strategists will be favored if they mate indiscriminately for all other fitness differences between potential mates. Now consider what would happen to an indiscriminate female if she were manipulated by male seminal peptides to have a longer latency to receptivity to remating. For a change in latency (horizontal axis) from zero to two, choosy strategists would be favored for fitness ratios of zero to .35. If the change in latency were from zero to five, choosy strategists would be favored over fitness differences from zero to .5.

This thought experiment shows that coy, choosy females could result from

male physiological manipulation of female behavior and its interactions with other time varying parameters. Choosy may not be a fixed and genetically determined, female-specific trait. Rather, social circumstances may induce choosy females, just as Natalie Angier and other feminists have thought for a long time. This sort of explanation for female natures deserves more empirical attention.

The ardency-coyness continuum

To evaluate whether males are more indiscriminate and eager about mating than females, my colleagues and I (Gowaty et al. 2002, 2003) studied three species of *Drosophila* in which asymmetry in gamete size varies. We began these experiments because one of my collaborators questioned whether the supposedly coy female *Drosophila pseudoobscura* would approach males in another series of experiments we were planning. To find out, we put one virgin male and one virgin female in a small shell vial and observed all of their movements toward or away from each other during the first five minutes of their exposure to each other. We defined "interest" as the proportion of an individual's movements that were toward the opposite sexed individual.

Of the three species we studied, *D. pseudoobscura* have the greatest asymmetry in the sizes of their gametes. Males ejaculate 25,000 very small (compared to other species) sperm. By comparison, the eggs of the females are gargantuan. Parental investment theory, therefore, predicts discriminating, passive females and indiscriminate, eager males. No statistically significant differences in "interest," that is, relative eagerness or passivity, occurred (Gowaty et al. 2003). Male discrimination could not have been due to sperm limitation, because all males were virgins yet old enough to mate, and none had ever before seen females. By using virgin females old enough to mate, we eliminated the possibility that previous exposure to males induced discriminating passivity. Because we observed no significant differences in female and male interest, we suspect that the "basic" natures of individual *D. pseudoobscura* are likely to be similar in environments like our tests. Our results surprised my collaborator, Wyatt Anderson, who remarked on seeing the videos that he would not have expected the females "to be so active." Since then, he has frequently said that he is "doing feminist science." I consider our collaboration exceptionally productive because one of the by-products of our sometimes different expectations is that our experimental designs are improved over what they would have been had only I been involved in their design.

We also studied *Drosophila hydei*, a species with very large sperm, so

large that sperm and egg are about the same size. Parental investment theory predicted no differences between the sexes in interest. Males were slightly, but statistically significantly, more interested in females than females in males. The individuals in our tests were virgins, so sperm limitation and/or manipulation of males by females could not account for these differences. An unexamined explanation remains for eager males in *D. hydei*. If males' survival probabilities were lower than females', selection would favor eager males in our experimental situation.

In our studies of *Drosophila melanogaster* males were more interested in females than females in males. But females were hardly "passive." Many showed interest in males equal to or exceeding male interest in them and many eagerly pursued their vial partner. Some of the males seemed as reluctant or more so than their female vial mates. Of the three study species, *D. melanogaster* was the only one for which observations on average matched the predictions of parental investment theory. They are consistent also with other alternatives including that females' survival rates may be greater than males'.

Conclusions

Feminism changed sex role science in at least four ways. First, it brought more women into our field. With them came their empathic responses (Hrdy 1986) to the lives of the nonhuman creatures they studied. More women scientists facilitated the birth of the "females' perspectives" movement. Many of those who use the female perspective language (Parker and Burley 1997) distance themselves from the politics of feminism while emphasizing their interest in the lives of females.

Second, as feminism grew, feminist consciousness spread among (male and female) scientists. Feminist consciousness nurtured the recognition of alternative hypotheses and underlying assumptions of reigning theories. It contributed to some scientists' insistence that the lives of females were worthy of study in their own right. Scientists self-conscious of their feminist consciousness were among the most active in calling for "reality checks" (Smuts 1992; Hrdy 1997): how could parental investment theory be reconciled with those enthusiastic females that solicited sex by sticking their flaming red perineums in the faces of males?

Third, feminism contributed ideas that scientists like me used to frame testable hypotheses.⁸ Hrdy might say that my modifications were part of the empathetic response that organizes so much social behavior research,

⁸ Gowaty 1981, 1983, 1992, 1996a.

whether by men or women. I may only be more self-conscious of my feminist consciousness. This brings an advantage, however, that is crucial both to the scientific enterprise and to another way that feminism changed evolutionary biology.

Feminism made the experimental designs better. Being self-conscious about my politics has helped to make my experiments better than they might otherwise be, because I institute a variety of controls that others might also use, and would no doubt use, if they were more aware of their own biases. One of my favorite ways is to collaborate with others who do not share my overt political commitments. The push-me-pull-yous that are inevitable when collaborators with different assumptions design experiments have saved me from experimental catastrophe more than once. And I exploit the good will and energy of undergraduates who know nothing of my hypotheses and predictions to make "blind" observations of the animals I would otherwise so enjoy watching. The cost of this experimental control is a personal loss in my own enjoyment; the benefit is better experiments through one of the best controls against bias ever invented.

A question that I cannot answer is why it has taken so long to incorporate females' perspectives into sex role science. In some ways it was not long at all in that Trivers's (1972) work was the first big, important step in the right direction. Trivers put females in the center of theory about social evolution one hundred years after Darwin's death.

I do not know why in evolutionary psychology the parental investment hypothesis is an axiom (Buss 1995), a foundation of evolutionary psychologists' enterprise. Perhaps they are so enamored of it because they are specifically uninterested in current fitness variation and/or fail to appreciate just how often selection probably favored flexible responses even in sex roles (Gowaty 2003).

I do not know why so few paid attention to Sutherland's work. For that matter, I did not recognize its importance until I began reviewing the sex role literature for this article. It was only after I "got" Sutherland that I forced my way through Hubbell and Johnson's mathematics. Perhaps others were as flummoxed as I was (though, of course, the math was really not so hard). Or, perhaps others did not think about their results in terms of sex role differences. Even Hubbell failed to recognize the significance of their results for flexible sex roles until I started asking him about them (S. P. Hubbell, personal communication, October–November 2001).

In the end, the lack of carefully designed tests of sex roles in typical species is the reason we do not know more. Empirically, the field moved forward with the experiments on pipefish and sea horses. I believe it will

again when experimentalists take advantage of the Hubbell and Johnson model to ask just how flexible sex roles are in species with female-biased parental investment patterns.

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A Conversation on Feminist Science Studies

In February 2002, Banu Subramaniam and I were participants in the conference "Balancing the Equation: Where Are Women and Girls in Science, Engineering and Technology?" sponsored by the National Council for Research on Women and the Barnard Center for Research on Women held at Barnard College. At this meeting, Banu and I were the only women of color on any of the panels. We were both struck by how many times this had happened to us since we first met in 1994. At that time Banu was completing her Ph.D. in evolutionary genetics at Duke, and she had been hired to start up a women in science program at the University of North Carolina at Chapel Hill. I was an assistant professor of the history of science at MIT. Banu and I have had many conversations over the years about the central questions addressed within feminist science studies; most specifically, we've shared a deep concern about the role and status of discussions about race/ethnicity within this field. The Barnard conference spurred us to talk about the problematic space we occupy in feminist science studies as women of color from diverse origins and perspectives. Currently, Banu is at the University of Massachusetts, Amherst, and working on a book, *A Question of Variation: Race, Gender and the Practice of Science*, which explores how experimental biologists can engage feminist science studies in the practice of science. I am currently completing a book titled *The Logic of Difference: A History of Race in Science and Medicine in the United States*, which reviews developments from the mid-nineteenth century to the present. Our exchange occurred by phone and via e-mail during the late spring of 2002.

Evelynn Hammonds (EH): Can you say why, as a postgraduate student in 1994, you became interested in feminist science studies?

Banu Subramaniam (BS): I think for me my entry into feminist science studies was very much in the realm of identity politics. I had grown up in India totally immersed in science and had wanted to work in biology since I was young. In postindependence India, science was central to the mission of a postcolonial India. The rhetoric of science that promised to

save us from the blindness of religious superstition and prejudice was one that I passionately embraced. What was so inviting about science was the promise that identity was irrelevant. A middle-class girl in the third world could have the same dreams and ambitions (and access, opportunity, and treatment, I believed) as any privileged white man in the West. I came with all those dreams and ambitions. Yet halfway through a graduate program in the United States, I began questioning my place in it. I was struck by both the visibility and the importance my identity was suddenly taking. It seemed that my identity was central to the social relations in my department since I was one of the few women of color and third-world women in the program. I turned to women's studies as a way to begin to understand that transformation.

EH: What were the texts that were the most influential for you?

BS: I think writings of Evelyn Fox Keller, Anne Fausto-Sterling, Donna Haraway, Ruth Bleier, Sandra Harding, Ruth Hubbard, Margaret Rossiter, Sue Rosser, Sharon Traweek, Helen Longino, Richard Lewontin, Nancy Tuana, Londa Schiebinger, Bonnie Spanier, and others, and of course, your work.¹ I think I avidly read anything that was out then. By that time you were already at MIT, right?

EH: Yes, I was already at MIT. When we met, Mary Wyer had invited me to Duke University to speak to women of color involved in science and engineering. As I recall, there was a lot of enthusiasm around my visit, in large part due to the Black Women in the Academy conference that I had coorganized at MIT during January 1994. I had finished my degree in the history of science after having completed a master's degree in physics at MIT and worked as a software engineer for five years. I started teaching at MIT in 1992. When I was in graduate school, in the history of science, during the late 1980s and early 1990s, the study of women in science was marginalized. There were no courses in gender and science taught at Harvard. I gave one of the first graduate seminars on these topics using Keller's, Harding's, and the first volume of Margaret Rossiter's books. Haraway's work was also getting a lot of attention at that time, though not in the history of science. So this period was really a formative moment for feminist science studies as it was becoming more visible in women's studies. At that moment a number of influential anthologies were published which were dominated by feminist critiques of biology.

In the meetings I was attending as a graduate student with the authors

¹ These texts included Hubbard, Henifen, and Fried 1979; Rossiter 1982; Keller 1983, 1984; Fausto-Sterling 1985; Levins and Lewontin 1985; Bleier 1986; Traweek 1988; Haraway 1989; Tuana 1989; Longino 1990; Haraway 1991; Harding 1991; Rosser 1992.

of all these texts I was struck by, number one, how much work there was on biology to the exclusion of all other sciences. I was especially disturbed by the lack of analyses on physics, which is what I had studied in graduate school before studying the history of science. And therefore I always had a sense of dis-ease that the critique was claiming to be a critique of science, but it really was not a critique of science. To me, it was a critique of biology, by and large. To my mind the most significant issue that had emerged by the early 1990s was, first, the multiple theoretical strands in what was called feminist critiques of science. Second, the tensions between feminist critiques of science, the history of women in science, and work on equity for women in science. Initially some people felt that these efforts were all part and parcel of the same project, though the explicit relationships between the theoretical work on gender and science, historical work on women in science, and equity projects for women in science were not clearly articulated. Yet despite this lack of articulation of the relationship between these efforts, many people began another set of projects which were designed to bridge these three areas of inquiry by bringing the feminist critiques of science to women in science groups. The idea was to use the feminist critiques of science to help women in science who worked on equity issues see more clearly where the locus of the problem of women's marginalization in science lay. Some attempts were also made to give the historical work on women in science a more theoretical underpinning through using gender as a category of analysis borrowing from the work on gender and science.

Now, I think even early on, the first time I thought there were possible schisms within this project called feminist science studies came in writing with Helen Longino, a philosopher of science, the article "Conflicts and Tensions in the Feminist Study of Gender and Science" (Hammonds and Longino 1990).² The article is bifurcated. Helen wrote about the feminist critics of science, the small number of them, and she did a survey of their work and discussed the tensions between them with respect to what they were trying to do in their work. And I wrote about the women in science question. We made no attempt in the article to ask the question why the two projects should be addressed together. I certainly could see the tensions, but neither of us could name them—and the article reflects our inability to do so.

BS: I remember it—the schism as well as my discomfort with the half on women in science. The two sections had completely different foci,

² Part 2 of this essay (176–81) was written by Evelyn Hammonds as identified in the text.

tones, methodologies, analyses, and languages. And I remember wondering if we'd ever learn to do that work together. I think both sections were asking very important questions, but it was as though we could not put the minds and abstract analytic critiques together with the bodies of the women who lived in the institution.

EH: And for me, I was still in a place of asking why women scientists found these feminist scholars' work so difficult to understand. Even though, since I was trained as a scientist myself, I knew my own dis-ease about the feminist critique being very, very weak with respect to the physical sciences and not extensive with respect to the biological sciences. And, because I had retrained to become a historian of science, I had begun to see that feminist critiques at that time tended to draw what I felt was a somewhat naive picture of the scientific landscape, broadly. But it was still powerful and intellectually interesting to me. And I actually thought we were sitting at the beginning of a process—we were going to be enrolling lots of people into this project, there was going to be a certain kind of leadership, and we were going to see the kind of explosion of scholarship that people saw in other fields, such as critical legal studies, literary studies, and historical studies.

BS: And I think that is the central formulation that has stayed with the field, that the feminist critiques had something to "teach" women scientists, as well as the fact that there is "a" science and therefore "a" critique that we can develop. It is funny to hear you talk about the excitement ten years ago, because many still retain the feeling that the explosion of the field is around the corner! Clearly this hasn't happened.

EH: No, I don't think it has happened. I really don't.

BS: Now when you're talking about this point in the early nineties, you were already an influential figure in the field, and you stood out. Do you see major shifts in the field and find the same schism now and the same sets of issues?

EH: Let me answer that in several ways. Let me define what I think the schisms are. I think the primary schism has been between the feminist critiques and women in science efforts. This is a point that Evelyn Fox Keller made many years ago. The women in science efforts are characterized by a set of initiatives, all over the country, to improve the conditions for women already in science and to remove the barriers that restrict the entry and retention of women in science. So it's overwhelmingly an equity project. It's a project that spans programs to encourage girls to do science from elementary and high school all the way up to graduate school. It also encompasses programs to improve the conditions of women scientists in industry and the academy. This set of projects has been encapsulated

in the pipeline metaphor, and we can return to this point and talk about the problematics of the pipeline metaphor.

Feminist critiques of science, on the other hand, represent a more amorphous kind of intellectual project in the sense that it could be identified with a certain set of texts and individuals emerging in the mid-eighties to late eighties and into the nineties. Here I'm talking about the work of Evelyn Fox Keller, Anne Fausto-Sterling, Ruth Hubbard, Ruth Bleier, Sue Rosser, Bonnie Spanier, Helen Longino, Donna Haraway, and others. In some ways, this project was never defined by these scholars as a singular project. The feminist critique of science became a singular project out of the ways in which the work of these scholars was brought together in sets of anthologies and through their appearance at various conferences on panels with titles alluding to a feminist critique of science. Many of these scholars did cite each other's work, however, and those citation practices also produced the project as a visible subfield in academic circles. However, though the work of feminist critics or analysts, if you will, gained greater visibility, it did not emerge from a single discipline like law, or history, or literary and cultural studies. So a second schism that I saw emerge was a kind of lack of internal coherence in an arena of scholarship. I must also say that perhaps *schism* isn't quite the right word to describe what was occurring.

BS: I think it became a singular subject because of how it got used in women's studies and the fact that there were clear resonances between all their works. It became a critique to reinforce the distinction that emerged about sex and gender early in women's studies, where sex was immutable and in the realm of nature/biology while gender spoke to the power of culture. It seems that the project of women's studies became a project of articulating gender, and the feminist critiques were used as a way to highlight the centrality of science in the construction of women's bodies and also became a way to dismiss and critique the power of biology/science. "The critique" (and it was referred to in the singular back then) became, I think, an easy and useful way for feminist scholars to learn and teach about science.

EH: Precisely. And so the "feminist critique" became incredibly influential because it offered the perspective about the way in which certain aspects of the biological sciences' study of nature were critically linked to the processes of naturalizing sex and producing something cultural called gender.

Therefore, it was influential in women's studies for just the very reasons you've said but as an independent entity or a project, and I really want to contrast it to, say, feminist literary studies or feminist legal studies, I

think it didn't cohere in a similar way. By coherence I mean that when one looks at this body of work and the kinds of intellectual conversations occurring at conferences, what one sees is a lot of cross talk. Scholars who work on women in science discuss the structures within scientific communities that have led to the marginalization of women in broadly sociological terms, while scholars working on gender and science focus on the gendered nature of science itself. Of course, I'm making a very broad generalization, but I don't think it is an unfair one. Just look at what happens at some of the conferences on women in science, gender and science. These conferences have continued to proliferate even up to today. And yet I would argue that they are plagued by what I'm calling a lack of coherence because in some notable instances they try to focus broadly on women, gender, and science without articulating how these are related.³ It's clear from the papers from an influential conference in Minnesota in 1995 that, a decade after many of the important texts had been published, the question of what the relationship is between the research on women in science and research on gender and science had still not been answered. More fundamentally, the various scholars involved in this meeting had differing perspectives on the definition of science itself. It was notable that, in this meeting, such questions were raised, even though few answers were presented.

BS: I remember going to that conference and being struck by the same schism. What was really exciting about the conference was that it was the first conference that I attended that was attempting to make these linkages. As someone who wanted to continue doing experimental biology, I was starved for locations where everyone was at the same table so we could talk about the relationship between these subfields. And at this conference, everyone was there, but never at the same table. So there were some panels that dealt with women and science and equity issues, girls and science and equity issues; then there were the panels on philosophy, sociology, history of science. To someone who was struggling with engaging both the equity questions and the questions on the construction of knowl-

³ See, e.g., the conference held at the University of Minnesota in May 1995, which brought together scholars who worked on "gender and science, scholars who worked on women's participation in the sciences, and scholars who worked on both aspects to explore the potential for synergy between two pieces of a larger puzzle." In an essay introducing a volume of a selection of the conference papers the organizers—a historian and a philosopher of science—asked the question that captures what I mean by the lack of coherence in this arena. The title of their essay was "The Women, Gender, and Science Question: What Do Research on Women in Science and Research on Gender and Science Have to Do with Each Other?" See Kohlstedt and Longino 1997, 3.

edge, this was very frustrating. So while everyone was there, these cross-linkages never got made, officially or unofficially, because ultimately everyone went to their subspecialty and the different groups were never in the same room.

While I prefer the term *feminist science studies* (because it allows the possibility of construction and collaboration in addition to critique), I think the formulation of the "feminist critique of science" is the mode that still works in most women's studies circles. Even today, most women's studies scholars engage with the sciences through these critiques rather than walking across campus to see what work happens in these fields today. In talking about these foundational texts, I am also struck by how current they still are. If we take the multiple anthologies on women/gender and science that have come out in the last many years, the majority of them still contain the same foundational texts that began the field. As a preparation for this conversation, I remember that you and I sat down with most of these anthologies—and they are all wonderful compilations—but all of articles were ten to fifteen years old. While these classic essays are wonderful, it is striking that we are still using this material and largely because these frameworks still speak so strongly to us. Issues (and critiques) of biological determinism, sociobiology, et cetera are as relevant today as they were twenty years ago. Certainly, evolutionary psychology seems like yet another incarnation of sociobiology. But I'm afraid this is because the idea of critique has taken root so strongly within women's studies that we cannot move on. While we talk about fighting binaries/dualisms, most programs (here I'm talking about the deployment of feminist science studies, not the field itself) reinforce the binaries of sex/gender, nature/culture by using feminist science studies to critique science/nature/biology until one is safely in the humanities/social sciences realm. In the last twenty years, there are only rare examples of collaborations where women's studies becomes a site for new knowledge "in" the sciences (although that is certainly the model of women's studies in every other discipline).

EH: Now, back to another question. Part of my "influence" in this field has to do with the wide dissemination of the article I coauthored with Helen Longino in 1990 and the equally widely reprinted interview with me that appears in many anthologies on women in science and feminist science studies.

I think I began to be "influential" because I've consistently raised the issue of race in these venues. Part of what I hoped to do by earning a doctorate in the history of science was to really try to understand, in the most complex way I possibly could, how race had been marginalized and

yet remained powerful both institutionally and epistemologically within science. When I began to study the history of science, there was very little work out there that explained in a complex way the relationship between race and science in racially and ethically diverse societies.

BS: What was your dissertation about?

EH: My dissertation (and subsequent first book) was a study of the control of diphtheria in New York City around the turn of the century. I used this case study to try and understand how developments in the new laboratory-based science of bacteriology were translated into clinical and public health practices to control a very prevalent and terrifying disease that largely struck young children. In many ways the book allowed me to explore themes that showed the complexities involved in applying new scientific developments in a context where people were desperately trying to save children's lives. This context—the complex politics in New York City, professional frictions between public health workers and physicians, the large and ethnically diverse population of the city that was skeptical of the claims and practices of scientific and medical experts—all shaped and constrained the application of bacteriological knowledge. Though the book doesn't explicitly address questions of race, this project helped me understand how to think about science, medicine, and public health and the relations between science and society in more complex ways. I've used these insights in my work on African-American women and the epidemic of HIV/AIDS and in my current project, which is a historical study of "race" in science and medicine in the United States from the mid-nineteenth century to the present.

BS: Now, race has a similar history within feminist theory and the development of women's studies as well. Do you see the invisibility or marginalization of race differently in the two fields?

EH: Yes, I do. One of the differences, and I think this is one that I lived, is that, within feminist studies more broadly, there were lots of voices from women of color who were challenging the ways in which the term *woman* had been defined in explicitly white, Western, and privileged ways.

The challenge from feminists of color, which is something I certainly shared and participated in, was very active, very strong, and led to, I think, a deep and profound reassessment of how feminist studies was to proceed. It made people really come to terms with the ways in which *woman* was not a singular category. This work has become very sophisticated and nuanced at this point, though I have to add that feminists of color have not paid very much attention to what is happening in science, and they've only given limited attention to medicine. One important exception is Patricia Hill Collins's article that argues that the notion of "intersection-

ality" could be a useful methodology for feminist science studies to employ to address race (1999).

One could say that, because of the small numbers of women of color in feminist studies of science, the work has not been challenged to grapple with race/ethnicity. When I came to science studies, there was no tenured scholar of color who took up the issue of race in very explicit terms or even discussed race as a concept to be understood in relationship to gender. Among the leading scholars, only Sandra Harding, Anne Fausto-Sterling, Londa Schiebinger, and Donna Haraway are notable exceptions. Haraway's work was especially significant because, in *Primate Visions* (1989), she locates the role that race played in the colonial project that primatology was deeply embedded in. But hers is the only extensive discussion of the ways in which race worked its way through the science of primatology and became part and parcel of how primates are described, how their behaviors are characterized, et cetera.

Since so few people writing about gender and science wrote about race and science, I was personally quite troubled. I was also concerned about how I was getting marked as the voice of the "raced other" simply by showing up at conferences. People were very receptive to my work, and continue to be very receptive to the kinds of things I've had to say about race, but I have yet to see many others address this topic in the history of science or feminist science studies. Anyway, I think it's different from other areas of feminist studies because there have been more women of color involved in producing analyses about the relationships between race and gender.

BS: And why do you think questions of race and women of color were so much more invisible in feminist science studies? Because of consistent marginalization and invisibility of women of color in both the scientific project and the feminist project?

EH: Yes, I think it's deeply connected to both. First and foremost of course is the small numbers of racial minorities at the top of American science. Second, there is little work in the history of women in science and in gender in science on women of color or race that can provide a ground for analysis. I can see structures of invisibility just proliferating across feminist studies of science. Even new work in science studies that addresses laboratory cultures fails to deal with race. I know of no studies of laboratories, beginning with Latour's work to more recent studies, that use race as a category of analysis. Laboratories and laboratory cultures are characterized in ways where race simply has no place, and therefore it is not seen.

Now, if you turn back to the women in science literature, though, and

the women in science effort, there you do see more attention to women of color but only in terms of their lack of access to the scientific careers. There are no linkages drawn between the status of women of color in "women in science" efforts to the projects on "gender and science" and "feminist science studies." Here again structures of invisibility proliferate—women of color are the most underrepresented group in all of science in the United States. Nationally, programs designed to help women in science have had a difficult time reaching women of color, while programs to help people of color in science have largely focused on men. So there are overlapping, interpolated, and unexamined structures of power relations that have allowed the feminist critique of science to proceed with the development of a concept of gendering in science that does not consider race and simultaneously allowed women in science projects to continue to put forward efforts to support women that likewise do not incorporate race or ethnicity.

It is these multiple structures of invisibility that produce an absence of a deep analysis of race. My project has always been to try to step into the space created by these structures, but by stepping into it, I almost became iconic in that I was in many ways and senses a singular voice. And even your presence and my presence on a panel reflected this space, as does the way that, when you would speak about race and I speak about race, you get figured as the Indian woman and I get figured as the black woman. I don't remember a moment when people actually saw or interpreted or responded to our presentations as if they were speaking to a similar project.

BS: Absolutely. What has this invisibility meant?

EH: I think the invisibility has meant that we still know too little about how the life sciences in particular produce "natural" facts about human differences. Though as Nancy Stepan and others have argued, analogies between race and gender were routinely invoked in the nineteenth and early twentieth centuries to frame women's so-called incapacities and racial minorities' so-called incapacities in similar ways by white male scientists (Stepan 1986). Sexual differences were framed in ways similar to racial difference, particularly in the use of the body as the *prima facie* evidence of difference. Yet these framings of human differences have had profoundly different social consequences and historical trajectories. To look at how the sciences of human difference emerged, evolved, changed, and achieved a certain kind of cognitive and social authority through the lens of race, gender, and sexuality can tell us a great deal about how science works variously in complex modern societies.

BS: Can you speak to what role an integrated project of race and gender

and sexuality, in terms of science's role in the construction of difference, might play in women's studies?

EH: For women's studies, if we were to teach about the sciences of human differences, I think the linkages between science and society can be made more explicit and useful for students to understand how science works. Certainly people have tried to do this in their classes, but I don't think such an integrated project has been the focus of important articles.

BS: Another area of lack of linkages is the connection between the development of difference in colonial and postcolonial contexts, the development and deployment of race in those contexts, that is, developing a global genealogy of race. While there are some excellent local histories of colonial science in different locations, we still need a better understanding of how they are connected to each other, how they circulate and recirculate globally, and how they have and continue to reinforce each other. As someone who works on race in the United States, how do you see this? Why is it important for us to make these linkages?

EH: Well, I think it's important because, number one, some of the postcolonial work is really strikingly sophisticated in articulating relationships between state policies and scientific policies and scientific practices and the transformation and re instantiation of scientific ideas in colonial and postcolonial contexts, in ways that are extremely helpful for those who work on the United States. And if you can write about the United States and its treatment of its native-born minorities with respect to the scientific enterprise, in similar ways that people do in postcolonial studies, then I think you can bring some sophistication into an area that is to date not very theoretically sophisticated. And it's important because I think it's necessary to decenter the Western scientific project. In other words, the rest of the world has been and remains necessary to the Western scientific project. But I would emphasize that this is a project that still deserves much more contextualized elaboration. And how one figures the agencies of peoples who are in those colonial contexts and postcolonial contexts are also projects that are much more complicated and that we need to elaborate. I see postcolonial studies as a rich resource for trying to understand the U.S. situation.

BS: Right. In talking to many people in the field about why these linkages don't happen and why everyone seems to feel they are working in isolation at their respective institutions, people cite the institutional difficulties in doing such cross-disciplinary work: which societies or conferences to attend, what intellectual groups and traditions already exist, disciplinary and departmental locations, et cetera. Do you think there are other reasons for these gaps?

EH: I think institutional issues are one part of it. I think a different part is how we might characterize the situation of the first generation of feminist science scholars. What did not emerge was a research program and a location for that research program. In other words, many of those scholars did not have graduate students who took up the questions that they were working through in their early texts. In contrast, consider women's history. The feminist scholars who were so involved in bringing women's history to the fore in the United States have produced legions of graduate students now. The Berkshire Conference of Women's Historians is full of these students, who have continued work on women's history and gender in history, so that it now is a burgeoning and thriving, identifiable field within historical studies and has become, in some respects, mainstream. Gender is now a legitimate category of historical analysis that graduate students who aren't explicitly working on it have to consider, in whatever historical topic they might be working on. I don't think feminist science studies, because of its lack of an institutional focus, has reached that point.

BS: Absolutely.

EH: Since you were just involved in editing a book on feminist science studies (Mayberry, Subramanian, and Weasel 2001) at this historical juncture, what is the project of feminist science studies? What was the process of that book? What did doing that book help you to understand about the project of feminist science studies at this point?

BS: It started with the three of us finding each other at meetings and feeling similar frustrations and concerns about our isolation in doing this work. I don't think any of us had ever been in an institution where there was more than one or two other people who would define themselves even vaguely as working in feminist and science studies, and, given that the field is huge, it seemed virtually impossible to find someone else who worked on things closely related to what we were doing.

EH: You said it was such a huge field. When you say huge, do you mean a diverse field?

BS: Yes and no. Certainly in terms of disciplinary, interdisciplinary foci, and approaches, it covers virtually all disciplines and interdisciplines. So, as someone who considers herself working in the field of "feminist science studies," I often find myself reading journals in mainstream biology, women's studies, science studies, history of science, philosophy of science, sociology, anthropology, literary studies, women's history, philosophy, cultural studies, ethnic studies, postcolonial studies, et cetera. It seems like an impossible task, and I can never keep up! In that sense it seems like a huge field. It is also diverse in terms of disciplines and approaches. I

struggle with keeping up with a multitude of disciplines, methodologies, and approaches, because interdisciplinarity seems critical, as these disciplines have important things to say to each other. We found that we constantly wondered what we'd missed because relevant articles appeared in so many disparate journals. So, we often relied on word of mouth to hear about them. What was striking was that there was no central journal, no central conference, no central group of scholars that consistently came together, ever. If I'm at the National Women's Studies Association meetings or the Society for the Social Studies of Science meetings, I would need to go through the index to see who was there and couldn't assume that a common set of colleagues would show up each year. It was never possible to say who identified themselves as being part of this field since there was/is no "field." Some scholars who I'd consider to be "in" feminist science studies might identify as historians or anthropologists and do not see themselves as part of the project of feminist science studies.

So for people just beginning to work in the area, it was something that was really frustrating. Yet, in looking at the literature, it seemed that there were a core set of ideas and a core set of critiques about how to view the institution of science and what the history of science said about the deployment of gender, race, sexuality, and class.

But, on the other hand, it was not as though anyone had ever articulated a "core" or that this was even a field of some kind. And so the anthology came out of wanting to make sense of that, to figure out how diverse this field was and in what ways it was diverse. Also, since many of us were junior scholars and recognizing that we constantly met new people who we'd never heard of, we also wanted to see who was out there and who was doing what kind of work.

So we put out a broad call for papers and waited to see what we got. We did not think of it as a comprehensive collection. And I think what we found was that the field was all over the place, but yet there was a recognition of a common project, of what people were trying to do. So they were each chipping away at a different part of it, in trying to understand it.

EH: What do you think the common project is?

BS: I think largely it seemed that it was about trying to understand the relationship between science and gender, race and class, sexuality, and colonialism, and recognizing that science has been a central force in constructing human differences and all of these categories. People were trying to understand the historical and contemporary role of science as well as exploring the interconnections between science's constructions of race, gender, class, sexuality. That, I think, is broadly how I read the field.

Now, exactly what that relationship was, how people studied it, how

people thought we should go about studying, seemed hugely variable. And so, within that large project, I would also put people working on women and science, including those issues of equity. And again, most contributors in the anthology had not heard of each other, and there was little communication between individuals who all see themselves as working on similar questions.

What was interesting to me about the responses we got were the difficulties of institutional location and professional development—everyone had their own unusual idiosyncratic development. In editing the anthology, the range of textual practices was immense, and getting them into an accessible format was a real challenge. What was most exciting to me was that there were scholars in the sciences who engaged with feminist science studies in redefining their projects in the sciences. Many of them were also “trained” in women’s studies in that they had taken courses or gotten degrees in the field. That signaled a real movement in the field and demonstrated that while the “schism” seems very visible in the field, there is a lot of movement among junior scholars. These boundaries of science/humanities, sex/gender are often transgressed in all kinds of ways, although they get little “press.” I found this aspect very inspiring and hopeful. The two large silences I could identify in the responses we got back are the ones you have mentioned: race; and the physical sciences and engineering.

The other thing that also struck me that relates to your earlier point (about writing the article with Helen Longino) is that many practicing scientists want a critical analysis of the climate and social life of scientists, that is, an analysis of how the history of science has shaped a climate that is still reminiscent of clerical Christian culture. And while there are certainly beginnings of such an analysis in the works of David Noble (1992), Evelyn Fox Keller (1984), and Sharon Traweek (1988), it is something we need to develop further. Interestingly, it’s your interview with Aimee Sands (1993) and Evelyn Fox Keller’s narrative about the women in physics (1977) that get used over and over again.

EH: Yes, and the various other anthologies that have now come out, or collections of interviews with women in science.

BS: Right. But I think a lot of those don’t seem to have a theoretical frame, and they’re written as narratives and stories (some of them very powerful) about the experiences of women in science. Whereas in yours and Keller’s, I think there are a critical set of issues about the culture and institution of science that are useful in a women’s studies classroom. So if you look at women’s studies courses or these anthologies, it is these two essays that get the most attention. It is interesting to me that, despite the

lack of attention to the physical sciences in feminist science studies, at the level of autobiography or experience, it's physics that gets overrepresented.

EH: Yes, that's a very odd thing.

BS: Yes. I wonder why the field has not engaged more fully with the question of how the historical, sociological, and philosophical development of science has shaped the role of gender, race, class, and sexuality in the social relations of science. Basically, I think that's the part of the puzzle that links questions of equity and women in science with gender and science or feminist science studies or whatever we want to call it. And I wonder to what extent concerns of essentialism and identity politics keep people from making those links. What strikes me again and again at different institutions I've been at is that you bring ten women in science together to talk—say, graduate students to talk about their experiences—and, inevitably, most of them will be telling very similar stories. I cannot count the number of times I've been in a room with other women scientists and we can name experiences (hallway conversations, incidents at parties, lab encounters, discussions with mentors and advisors) that are identical. Some of those moments really feel like *The Twilight Zone*! There is a haunting homogeneity of scientific culture, not only across departments but also across institutions. So, for example, the rampant insecurity and constantly feeling like an "imposter" (of not being as good as the institution thinks you are); the familiar speaking up but never being heard (until it's repeated by a male); the constant commenting on attire; male students often getting time with male faculty through playing sports and socializing after classes; the assumption that you'll get a job because you are a "woman," and especially "minority woman."

In working with women scientists, explorations of experience and identity have a deep resonance and power. However, within women's studies, experience and identity have become deeply problematic, and with good reason. Also, claiming an identity of any kind is deeply problematic in the sciences, and there is a long history of scientists refusing to make any claims of gender or race or that these categories matter in scientific careers. And so, the challenge within feminist science studies is to be able to account for the homogeneity of women's experiences in scientific culture while avoiding the pitfalls of essentialisms that further marginalize women scientists or scientists of color. When you listen to scientists, the scope of experience—and how ossified scientific practice seems to be in certain places—is rather striking (even though it's extremely diverse and heterogeneous in others).

EH: Well, I think this is a central problematic in the sense that I would

want to see really deeply contextualized stories or narratives and analyses of a variety of scientific communities and cultures over time. And analyses that show how power relationships and dynamics and processes operate within those cultures over time. It's really picking up on some of the things that Sharon Traweek's work pointed to in her careful study of the high-energy physics community: What do differently situated scientists in different scientific fields understand gender differences to mean, or racial differences to mean? What are the effects of a kind of homogenizing manner of dress, speaking, reporting of ideas, et cetera, that seems to be so fundamentally a part of scientific cultures?

We still need more work on these issues. What happens, though, with the women scientists when they get together, as you say, is that they start telling stories about their experiences in science, but there's no analysis of the stories.

BS: There's no analysis by them beyond the persistent question that emerges: Are my experiences really about gender? Doesn't what happens to women happen to everybody in science? Is that story really about race? Doesn't that happen to white people, too? I agree there's this complete absence of analysis of those stories. I think it's a failure of feminist science studies. Part of the reason for the schism you talked about is a failure of giving a good analytic account of why these stories emerge repeatedly.

EH: I agree. In that sense, feminist science studies could offer a great deal to the literature on how to theorize experience. In the literature on women in science, from the policy perspective, one of the things that I see happening is a slow convergence toward questions that feminists have been raising. For a while policy analysts focused primarily on documenting the absence of women and minorities in science and on generating recommendations to institutions on how hiring and other practices should be changed. In my view these same policy analysts are now saying, "now that barriers to scientific work have been reduced and we've provided monetary support and other resources for women scientists and minority scientists, we still can't explain why women or minorities are not 'fitting' into scientific cultures and thriving and becoming leaders." As a result many policy documents now end by stating that it is time to look more carefully at the culture(s) within science in order to understand more completely how those cultures are shaped by the presence or absence of women and minorities. These are issues that have long concerned feminist scholars.

Now I want to turn to another site that needs to be examined, which is how we experience meetings like the recent one at Barnard College on "Balancing the Equation: Where Are Women and Girls in Science, Engineering and Technology?" and how the use of the pipeline metaphor

has shaped such gatherings.⁴ The Barnard conference was another meeting where research on women in science was brought together with research in feminist science studies as a kind of instantiation of the pipeline metaphor. According to one scholar the pipeline metaphor was used initially at the National Science Foundation (NSF) in the mid-1980s as a way to capture and characterize the present and future supply of scientists and engineers. It was first applied to the problem of the underrepresentation of women and minorities in science and engineering in 1987, when the then director of the NSF used the metaphor to argue for increased appropriations for NSF. Where past invocations that it was just and right to foster the development of women and minorities in science had failed, the pipeline metaphor proved to be an extremely powerful way to link the underrepresentation of people in these groups to issues of national competitiveness in science and engineering. Thus the problem of women and minorities in science was transformed into a problem of economics and national security.⁵ That questions about the status of women in science are so deeply caught up in this pipeline metaphor is very troubling because it means that, at conferences, women's work on a range of issues—girls' science education in elementary school, intervention programs for girls in high school, intervention programs for undergraduate women, problems with graduate students, post-docs, and then the professorate—are treated as if they constitute one problem. Such configurations presume that women scientists who are practicing scientists at the top of the professorate have important things to say to someone who's trying to encourage young women in K-12 education. I have begun to experience these meetings as being trapped in the pipeline. I experience this metaphor as completely limiting and bracketing conversations and research agendas. The pipeline metaphor is one that is evocative, and so it provokes statements such as "the pipeline is leaky at one spot or another." Yet it doesn't ever provoke a set of questions that ask what this pipeline metaphor tells us about our conception of the scientific enterprise.

BS: Exactly. And I think this is the other part of the schism where it plays out. There is little response or concern to questions such as "Where is this pipeline coming from?" and "Where is it going to?" "Who laid these pipes?" "How is it embedded in global capitalism?" "Who are we

⁴ The pipeline metaphor is very prevalent in the literature on women in science. So it is common to see efforts to "increase or widen the pipeline" or to fix the "leaky pipeline," etc. The conference was held on February 7, 2002, and was sponsored by the National Council for Research on Women and the Barnard Center for Research on Women.

⁵ See Lucena 1996.

producing for what purpose?" "Why are we so invested in shoving all these young girls and women into the pipeline that is dark and dingy and not very habitable?" So while those working on gender equity are working on the access of everyone into those pipes, others are questioning the whole idea of that metaphor and its underlying conception of science and asking whether our project should be about getting more women into the pipeline or dismantling the pipeline.

EH: Precisely. And that's a thorny and a political question. First, from the policy perspective, little is said about men in science being in the pipeline, except for minority men. It's a kind of engineering metaphor that doesn't apply to white men. The assumption is that white men in science are brought into it, developed, and their interest in it nurtured from an early age all the way to the top of science. The use of the pipeline metaphor suggests that analysts don't need to ask whether this metaphor accurately captures how white men become scientists. Second, the questions of who made the pipeline and who the pipeline is for point to other issues about the characterization of the scientific enterprise that need to be addressed. And, of course, when we feminist science studies scholars start asking such questions, inevitably we are led to ask questions also about the role of capitalism and industry relations, the role of the state, and the funding of scientific education and scientific work in the United States, which lead to a much more complex analysis.

The way the pipeline metaphor is deployed continues to focus on getting people into it and making them fit. That's all it does. And if women or minorities don't fit, maybe the government will try to figure out what else should be added to the pipeline; but such analyses never question why the pipeline works the way it does, nor do they tell us anything about the context in which this pipeline is situated.

I don't know if in other areas of feminist studies novelists and literary scholars get together on the same panels at conferences. I don't know if practicing lawyers and feminist legal study theorists get together on the same panels to discuss going to law school and the problems of gender and law, as well as look at issues related to gender in the practice of the law. I mean, it may happen, but it seems to me that what happens in feminist science studies is a somewhat unique and often strange situation that brings feminist science studies scholars together with people who work on women in science, as educators or policy people or heads of programs, for a situation that, as far as I'm concerned, is simply one of cross talk. The feminist science scholars want to talk about what gender means, how gender functions, what race difference means, and how race functions in relation to science. We want to talk about social structures,

we want to talk about the context in which scientific communities work, we want to talk about power relations. We want to think about how science works within complex societies.

By and large, women scientists want to talk about effective programs, and science educators want to talk about role models. Therefore what occurs is for me a predictable performance by all involved. The women scientists tell the feminist science studies scholars that they don't understand what they are talking about. "You're talking jargon, you're talking over our heads," or they say, "What you are saying has nothing to do with what I have to do every day in my lab." On the other hand, the science studies folks are often defensive about their methodologies and the questions they want to address.

Putting us all together on one panel says that one function of feminist science studies work is to explain to women in science why their situation is what it is. And, for the most part, when we're sitting in these situations, we can't explain it, nor was that our project in the first place. Yet they look to us for an explanation. I find, often, that many times people are disappointed. Or it makes me feel that I should tell a story that allows people to relate to me in a personal way, that allows some women scientists to connect to my experience in physics. Certainly, many women scientists of color, African American and Latina, connect to my articulation of certain experiences that they share. So we bond around those experiences. But the next step, where I would want to go to ask what our shared experiences say about science, is by and large misunderstood.

BS: Yes. And unfortunately over the last ten years, as the political climate has become more conservative, it is difficult to get women in science projects funded that are overtly "feminist." And it seems that we've come to a current situation where a lot of scholars and feminist science studies are sick of this question, are tired of having talked and worked with little impact; they don't think it should be the project of feminist science studies to talk about the women in science question anymore, and they've done it for fifteen or twenty years, and they're done. And yet, nothing has moved. So, why do you think that is, and what makes the change?

EH: I have no doubt that there is a need for people who work on women in science to do that work. But I don't think that it's productive anymore for those of us in feminist science studies to participate in that work as people who are trying to do feminist science studies. I think we can participate in it as people committed to women in science.

In other words, I'm suggesting that I need to live more consciously inside my multiple identities as a scholar trained in science and as a historian of science. I only want to participate as a woman in science, or a

former woman in science, who has something to say about intervention programs and policy questions that are emerging. What I've done is try to say, in settings such as the Barnard meeting, that all of us should be wary of representing the current situation of women in science as a watershed moment, because we need to pay attention to history. When we pay attention to the history of women in science in America, as Margaret Rossiter's and others' work has shown, then it is not so clear at this time as to what has made so great an advance possible for women or minorities and what is stalling continuing advancement. So I try to speak, as a historian who's taking a long view, on the question of women in science, but I'm refusing to speak, again, as a feminist science scholar who's interested in certain other kinds of questions.

BS: That is interesting, and I think we disagree here, because this is precisely the question that engages me the most, because I think feminist science studies was personally so transformative for me. It allowed me to understand why, for example, scientists are supposed to be asexual, antisocial, or certainly not endowed with good social skills, singularly dedicated with no personal life, even a little crazy, and spending every minute in the lab, not paying attention to clothes or appearance. These judgments of good scientists came once from a clerical tradition that was in the service of God and now is in the service of science and finding the truth about nature and the universe because the roles of religion and science have shifted. It seems to me that, until we historicize and situate the practice of science (and why women and people of color don't fit in it), we will never be able to get beyond the myth that these arbitrary codes of "good" scientists are really the best way to produce "good" science. So, for me, feminist science studies has a tremendous lot to say to (and learn from) practicing scientists, and I really want to work toward a collaborative vision of women's studies/sciences. I really struggle with continuing to do experimental biology within women's studies programs. It is not as though we only have to "teach" scientists—we also have a lot to learn. Ultimately, knowledge of our racialized and gendered bodies shouldn't be entirely in the realm of the sciences. We can and should be coproducers of such knowledge. After all, if within women's studies we can have anthropologists, historians, philosophers, writers, and artists, why not scientists?

EH: I don't think we disagree quite so much. I am interested in feminist science studies for the same reasons you just mentioned. I'm just interested more now in finding more effective and productive venues to talk about the critical points you just raised. I just don't think forums where the cross talk dominates are the best sites to explore your questions. Questions, by the way, that I still desperately want better answers to as well.

I don't think that feminist science studies can develop further without more focused venues for exchange. You mentioned that we don't have a journal, that there's no central conference that everyone attends every year. I want to bring people together to help my thinking, to get some push back from people who are as immersed in these sets of questions as I am and we are, to see where we are, what we're doing, why we're doing it, and what it is we want to do. What kind of narratives do we want to tell? What kind of theoretical positions are we trying to push up against? And what do we want to develop?

To end, I want to return to one of our earlier exchanges about the "coherence" or lack thereof in feminist science studies. I wouldn't want people to read our exchange and go away thinking that we or I am suggesting that feminist science studies should be unified. I believe that the diverse perspectives in the disciplines from which scholars in this field come is one of its great strengths. And in many ways the divisions we have marked are not that different from other divisions in the highly contested field of science studies. Our conversation has pushed me to think that I need to think harder about what the cross talk we've been discussing is about. And it might be a very good place to begin to understand how the tension between women in science projects and feminist critiques of race, gender, and scientific knowledge replicate or challenge the authority of science in modern societies.

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Rachel Carson Died of Breast Cancer: The Coming of Age of Feminist Environmentalism

Feminist environmentalism is shifting paradigms in public health, political economy, philosophy, science, and ecology. Feminist environmental theory and women's on-the-ground ecoactivism are challenging and transforming approaches to a breathtakingly wide range of issues, from animal rights to the environmental economy of illness and well-being, from exposing and theorizing complex processes such as global ecopiracy to interrogating the distortive privileging of "science" as an arbiter of the state of the environment. Feminist environmentalism is hot, and getting hotter.

But this may be news to most people outside the field. The one thing people typically seem to know about feminist environmentalism is that it involves something called *ecofeminism*, which they vaguely associate with spirituality and earth-loving women. From the outside looking in, feminist environmentalism can look like a large tent occupied by an elephant almost as large. Even from the inside it can look that way too. It is impossible to do work under the rubric of feminist environmentalism, or even to talk about it, without first explaining or positioning oneself in reference to ecofeminism.

Ecofeminism

Among scholars and activists closest to the project there is little agreement on what ecofeminism is or what its relationship is to the (presumptively) broader endeavor of feminist environmentalism. Browsing the literature we find *ecofeminism* variously described as a political stance, a take-it-to-the-streets movement, a feminist spiritual affirmation, an inspirational wellspring for women's activism, a retrieval of womanist earth wisdom, a feminist theory, an applied scholarship, a feminist rebellion within radical environmentalism (Sturgeon 1997, 31), an oppositional positionality, a praxis, and a remapping of women's relationship to place and ecology.

Despite a long trail of ecofeminist footprints on the U.S. activist and

academic landscape—almost three decades' worth, depending on where you put the head-of-the-trail marker—there is still little agreement on what it is, what its import is, and what its future is. As Noël Sturgeon (1997) wryly remarks, a concept that is invoked to “usefully if partially describe the work of Donna Haraway *and* Mary Daly, Alice Walker *and* Rachel Carson, Starhawk *and* Vandana Shiva . . . is a shifting theoretical and political location that can be defined to serve various intentions” (24; emphasis mine).

Despite this plasticity, the very term *ecofeminism* typically invokes strong reactions—and generally precipitates a rush to “for” or “against” camp making. There are three touchstone issues that separate ecofeminism-embracers from ecofeminism-distancers: the prominent association of ecofeminist thought with womanist spirituality, the (putative) essentialism of the ecofeminist affirmation of a meaningful nature-woman connection, and the old gown/town split between the presumptive sophistication of theory building and the presumptively atheoretical naïveté of social movement and activist practices.

Debates around these issues were once fresh and exciting. Indeed, what we might consider to be protoecofeminist insights played a more significant role than is often acknowledged in shaping the overall development of second-wave feminism in the United States. Second-wave feminists cut their political teeth on debates over the “unnatural divorce of spirituality and politics” (see, e.g., Bunch 1976; Iglehart 1978) and over fierce dissections of the existence and meaning of “female/male, nature/culture” dualisms (Ortner 1974). Feminist environmental issues, often refracted through the lens of peace issues, anchored the 1970s radical feminist debates on separatism, identity, and women’s activism that, in the United States, swirled around key figures such as Mary Daly, Susan Griffin, and Ellen Willis (see Daly 1978; Griffin 1978).

As conceptualized by ecofeminist pioneers such as Ynestra King, ecological feminism held promise as a bridge across the analytical divide between radical cultural and socialist feminism. King identified ecofeminism as a “third direction,” neither severing the connection between woman and nature (as socialist feminists would have it) nor reinforcing it (as many cultural feminists did): “the liberation of women is to be found neither in severing all connections that root us in nature nor in believing ourselves to be more natural than men” (1981, 15). Early articulations of the intersectional and interdependent oppressions of ecology, race, sex, and class (e.g., Reuther 1975) pointed to a path that transcended the dichotomous rendition of the human/nature relationship, the classic subject/object

split at the heart of Western philosophical inquiry. By the late 1970s, then, ecofeminism was on a roll, full of promise and intellectual excitement.

The first discernibly coherent feminist environmentalism emerged in the United States through these broader feminist debates and explorations. In tandem with antimilitarist movements and a then-nascent environmental movement, feminist environmentalism emerged as a fusion of powerful analytical and paradigmatic challenges and activist energies.¹ Environmentalists provided baseline insights into the interdependence of human life and planet life and offered a systems analysis of the ways ecological destruction cascaded through intertwined social and ecological webs. Feminists honed these understandings with analyses of the ways the construction of social power, in its ineluctably gendered dimensions, produced those conditions of ecological threat.

Thus, by the early 1980s, feminists were considering whether nuclear power and strip mining might be "women's issues." Feminists stretched extant analyses of the exercise of patriarchal and capitalist power to accommodate an interrogation of the deployment of violent life-threatening and planet-threatening technologies and modalities. The "Unity Statement" for the Women and Life on Earth Conference held in Amherst, Massachusetts, in 1980 (an event widely acknowledged as germinal in the emergence of U.S. ecofeminism) reads in part:

We are women who have come together to act on a common hope in a fearful time. We enter the eighties with alarm for the future of our planet. The forces that control our society threaten our very existence with nuclear weapons and power plants, toxic wastes and genetic engineering. A society and world economy organized for the profit of a small number of white men has created the conditions for wide-spread unemployment, violence at home and in the streets, oppression of third world peoples, racist attacks, inadequate food, housing and health care, and finally, the ecological devastation of the earth. We see connections between the exploitation and brutalization of the earth and her people and the physical, economic and psychological violence that women face every day. We want to understand and try to overcome the historical divisions of race, poverty, class, age and sexual preference that have kept women apart and politically powerless. Our concerns are many, but understanding

¹ One of the most comprehensive overviews of the emergence of ecofeminism in the United States is Sturgeon 1997.

the problems that confront us helps us imagine how we would like to live. (Women and Life on Earth 1979)

These were heady analytical and movement convergences. But even as ecofeminism emerged, it was already coming apart at the seams.

For many women, the eco-focused feminism emerging in the 1980s was necessarily rooted in a reawakening of earth honoring and earth caring, involving a rehabilitation of nature-centered traditions and invoking anew the salience of earth goddess, women-wise spirituality. For these women, the central project of what they called ecofeminism was reclaiming the sacred and celebrating women's nurturing—and special—relationship with earth forces and life forces. Ecofeminism put spirituality, earth goddesses, nature/culture identities, and debates about essentialism, antiessentialism, and maternalism on the feminist front burner.

As this school of thought emerged under the rubric of ecofeminism, a peculiar elision occurred. Inexplicably, even as the association of *ecofeminism* with a spirituality-inclined school of thought became more fixed, that word also remained in circulation as a term to refer indiscriminately to all manner of feminist environmentalisms. The word *ecofeminism* thus became a dual signifier, both meaning the specific spiritually centered school of environmental thought and also being used as a generic term for all feminist environmentalisms.

This meant that by the late 1980s, *ecofeminism* had become a fighting word. For every woman who reveled in the association of ecofeminism with earth goddesses, there was one who winced. Many women rejected ecofeminism, particularly academics in the social and biological sciences whose engagement with environmentalism was forged in a rationalist tradition, and who feared that talk of goddesses and life forces would undermine their hard-won but precarious professional credibility. For many political feminists, *ecofeminism* was a word to define *against*; the spiritual side of ecofeminism was derided as mystical bunk, dangerously apolitical and atheoretical. In 1993, Carol Adams calmly assessed the divide in this way: "There has been no one perspective on the place of spirituality in ecofeminism. . . . For some, the spiritual aspect of ecofeminism is integrally a part of their ecofeminism. For others spirituality is thought to derail the ecofeminist engagement with social conditions and political decisions that tolerate environmental exploitation, encourage unbridled consumerism, and fail to rein in military spending" (4).

The contributions of ecofeminism to feminist environmentalism are myriad, and ecofeminism itself is clearly an enduring part of the feminist environmental mix. Spiritual engagement with the fate of the planet brings

many women to the environmental table—and to the environmental barricades. Contemplation and contestation of the issues provoked by ecofeminism have produced a robust and challenging literature: on anthropomorphism, on the “sex-typing” of the planet, on encounters between feminism and deep ecology, on the nature of nature. The philosophy of ecofeminism is a well-developed field (see, e.g., Plumwood 1993; Salleh 1997; Warren 1997, 2000). Ecofeminist interrogations of spirituality, essentialism, and nature have generated smart, sharp, often witty feminist environmental writing, including now classics such as Sharon Doubiago’s rant at deep ecologists, “Mama Coyote Talks to the Boys” (1989), and Yaakov Garb’s (1990) provocative deconstruction of space images of the whole earth as “pornographic.” The ferocious back and forth between ecofeminist “factions,” and between feminists and deep ecologists, much of it waged in the pages of *Environmental Ethics*, was compelling reading—even juicy, in a mild academic way—throughout the late 1980s and mid-1990s.

But feminist internecine debates about ecofeminism also have been downright nasty, and many women have been deeply wounded by the exchange. The debates have also become inward turning (debating the debate as much as anything else) and, in my view, counterproductive to the larger enterprise of putting and keeping environmentalism on the feminist agenda and feminism on the environmental agenda. Feminist environmentalism has become bogged down in tiresome “pro-/anti-ecofeminist” reprises: Is the earth our mother? Are women closer to nature than men? Should they be? Should we decry the cultural association of women with earthy nature as a patriarchal contrivance or celebrate it as a privileged positionality? Is ecofeminism even feminist (or is it a complicity with the patriarchy that would inexorably bind women to nature, to the disadvantage of both)? Is the disparagement of ecofeminism by (many) academics reductive elitism (and a complicity with the patriarchal appeal to masculinist reason that would separate women from a meaningful relationship with their sacred earth origins)? We have interrogated these questions to the point of exhaustion, and well past the point of diminishing intellectual and political returns.

Beyond ecofeminist debates: Commonalities and agendas

In 1997, Noël Sturgeon argued that despite the travails, the term *ecofeminism* retained salience, especially as understood within the larger feminist project of “naming”: “Why ecofeminism? Why not just call the feminist analysis of the interaction between sexism and environmental problems

'feminism'? I believe that 'ecofeminism' as a term indicates a double political intervention, of environmentalism into feminism and feminism into environmentalism, that is as politically important as the designations 'socialist feminism' and 'Black feminism' were previously. Perhaps it is a name that will only be transiently useful within our history; but the stakes in such a politics of naming are deeply embedded in a long tradition within the development of U.S. feminism" (169).

I think Sturgeon was right—then. But I now detect the end of the useful era of transience. Feminist environmentalism is a mature enough field to move beyond the ecofeminist debates. It needs to. A substantial agenda—which in this new world order is infused with a certain sense of urgency—commands the attention of feminist environmentalism.

Moving beyond the ecofeminist debates, we find that most feminists who pursue scholarship and activist work on the environment—whether from "ecofeminist" positions or not—share common interests, among them a commitment to illuminating the ways in which gender, class, and race mediate people's lived experiences in local environments; an interest in examining the ways in which human-environment perceptions and values may be mediated through "gendered" lenses and shaped by gender roles and assumptions; an interest in examining the gendered nature of the constellation of political, economic, and ecological power in institutions that are instrumental players in the state of the environment; and an interest in exploring the interconnectedness of systems of oppression and domination. The best of the recent feminist environmental scholarship engages with and extends transnational, postcolonial, and poststructuralist deconstructions and challenges.

Animal rights and feminist environmentalism

Animal rights, for example. Feminist work on animal rights builds on the foundational ecofeminist effort to understand linkages between environmental oppressions on the one hand (such as speciesist hierarchies, or the hierarchy of value established through the commodification of nature), and human social oppressions of many kinds (such as those based on class, or race, gender, and sexuality classifications; or judgments of "value" attributed to physical ability) on the other. At the same time, the serious contemplation of animal rights makes a considerable contribution to destabilizing identity categories and adds a new dimension to theorizing the mutability of identity.

Feminist environmental scholarship and grassroots activism on animals pivot around three concerns: elucidating the commonalities in

structures of oppressions across gender, race, class, and species; developing feminist-informed theories of animal rights; and exposing the gendered assumptions and perceptions that underlie human treatment of nonhuman animals.

Like nineteenth-century racial and gender taxonomies that were constructed and then frantically repatched to keep pace with contravening evidence and with shifts in social and economic realities, efforts to fix a firm line between "us" (humans) and "them" (animals) are similarly becoming increasingly frenetic as the old standard-bearers of asserted human/animal difference topple. For example, the insistence that animals do not feel pain—until very recently the central subterfuge of vivisectionists and other animal experimenters—has been all but abandoned in the face of overwhelming contrary evidence. In response, the contested terrains have been shifted away from a simple "pain"-threshold test of animal rights to arguments about whether animals have consciousness or social awareness, whether they feel or express abstract emotions, can feel loss and deprivation, and the extent to which they demonstrate cognitive skills, curiosity, and problem-solving capacity, whether their behavior is motivated more by "instinct" or "intelligence." (Not unimportantly, this last debate is taking place even while the measurement and very concept of human "intelligence" itself are increasingly called into question.)

The myriad specific justifications for the oppression, enslavement, and exploitation of animals (which is occurring on a massive scale) are all rooted in dual assertions: of significant human/animal difference, and of the putatively scientifically provable "lesser" intellectual (or even emotional) capacities of animals. These are achingly close reprises of the conceptual bases for racial, sexual, and gender hierarchies. Echoing through the debates about animals are unmistakable invocations of familiar racist and sexist ideologies about "natural affinities," categories authorized by nature, destinies inscribed in biology, and "scientific proofs" of the limited capacities of the "other" that have rumbled through the centuries to justify slavery, the oppression of women, and ethnically and racially based holocausts and genocides. Two early feminist works remain unsurpassed trenchant analyses of these parallels: Marjorie Spiegel's comparison between animal and human slavery, *The Dreaded Comparison* (1988), and Carol Adams's treatise on the *Sexual Politics of Meat* (1990). A newer work by geographers Glen Elder, Jennifer Wolch, and Jody Emel (1998) adds an important postcolonial and place-sensitive analysis of the ways in which animal-human relations are used in the representational politics of cultural difference and in the production of notions of hierarchies of "civilized" societies (the contrast drawn, e.g., between "savages" who

ritually sacrifice animals vs. "civilized" societies that kill animals, but in a more modern industrialized way).

Out of struggles against the artifices of race, gender, and sex categories, feminists have developed sharp analyses of the distortions of binary dualisms and of the falsity of "science-based" identity categories. Furthermore, feminists have been in the forefront in exposing the sham of "universal" social hierarchies that are patently driven by culture-, gender-, and race-specific values. These insights are central to feminist reimaginings of animal rights.

Since animal exploiters rely on tropes about animals being "different from" humans (and thus not protected by human-like considerations or rights) and about animals being "lesser than" us in myriad ways, animal rights activists often start by arguing the opposite. Complacency about human exceptionalism is challenged by every report of parrots who can count, of whales with globe-spanning languages, of elephant mourning and memory, of gorillas who acquire extensive sign language vocabularies, of cephalopods who solve spatial problems, of cows who escape slaughterhouses with prodigious feats of athleticism and cunning, of lifelong devotional pairings between birds, of ants who form intentional alliances in supercolonies that stretch across hundreds of miles, of remarkable feats of dolphin intelligence. Recent discoveries that genetic differences between human and most nonhuman animals are slight (even negligible) have opened a new dimension in animal rights debates.

Such assertions of similitude between human and nonhuman animals are theoretically and philosophically congruent with the larger feminist project of destabilizing identity categories. Thus many animal rights feminists aver that the "line" between human animals and nonhuman animals is more of a broad, smudgy band than a sharp demarcation. Feminist and queer theorizing has blurred the line of "authorized by nature" identity categories. Feminist animal rights environmentalism queers the line even more. The "science"-based ideology that creates metrics of human/animal "difference" in its own image and then uses those metrics as if they were neutral analytical tools is the same value-laden universalizing science that puts gender, sex, and sexuality identity into discrete hierarchically stacked boxes—and it has been widely discredited, particularly in feminist theory. This is what Donna Haraway calls a moment of "boundary breakdown": "the boundary between human and animal is thoroughly breached. . . . Movements for animal rights are not irrational denials of human uniqueness; they are clear-sighted recognition of connection across the discredited breach of culture and nature . . . the line between humans and

animals [is reduced] to a faint trace re-etched in ideological struggle" (Haraway 2000, quoted in Kirkup 2000, 52).

Calling into question the premise and measurement of the human/animal divide, though, is complicated by deeply theorized feminist political commitments to respecting and retaining the integrity of "difference." In this, feminists part company with prominent male animal rights advocates such as Peter Singer (1975) and Tom Regan (1983), who argue for an extension of the moral community to include animals primarily on the basis of their sameness to humans. The examples above, of animals learning to speak and count, that seem at first so unsettling, are, of course, yet another way of valuing animals only to the extent that they meet or mimic human tests of "intelligence" and behavior. In this light, I might reframe my prior point: an animal rights philosophy that creates metrics of human/animal sameness in its own human image and then uses those metrics as if they were neutral analytical tools is flawed by the same presumptive universalizing that has so distorted our understanding of sex, sexuality, and gender—and should therefore be rejected. Philosophers Karen Warren (1990), Deborah Slicer (1991), and Val Plumwood (1993) elaborate most clearly the importance of developing a feminist animal rights theory that does not sanctify the "erasure of difference," an erasure that almost always works primarily to the advantage of the dominant class. Internationally prominent environmentalist Vandana Shiva presents a key feminist assertion that "even the tiniest life form [must be] recognized as having intrinsic worth, integrity, and autonomy" (2000, 74).

Bioengineering, with its potential to elide all sorts of biological difference, throws feminist animal rights theorizing into contentious debate. Extending her point about the necessity of recognizing the worth of animals in their own terms, Shiva, for example, chides Western feminists for playing with the postmodernist idealization of transgenicism. Directly challenging Donna Haraway's "cyborg feminism," Shiva argues that measured against the real-world impact of transgenicism on food production and lived ecologies, fantasies about "border crossings" (between humans and animals, or among nonhuman animals themselves) are an indulgence. The intellectual pleasure of playing with ideational subjectivities, Shiva says, is a Western luxury that ignores the harsh realities of the bioengineering assault on the integrity of animal identities:

The mad cow is a product of "border crossings" in industrial agriculture. It is a product of the border crossing between herbivores and carnivores. It is the product of the border crossing between ethical treatment of other beings and violent exploitation of animals

to maximize profits and human greed. . . . Species boundaries between humans and cattle are also being crossed to create pharmaceuticals in the milk of factory-farmed animals. . . . These border crossings, promoted by corporate elites for profit, are rationalized by the popular postmodern stances taken by some academics. . . . This [Haraway's defense of transgenic border crossing] academic rationale for an attack on environmental and Third World movements is based on false assumptions. . . . The mad cow, as a product of border crossings is a "cyborg" in Donna Haraway's brand of "cyborg feminism." (2000, 72-75)

This particular challenge has not been well developed elsewhere, but it will become increasingly important as genetic manipulation of plants and animals escalates.

Most feminists in the forefront of developing and advancing animal rights theory locate the primary challenge of their work against the positions of Singer and Regan, the two men whose work largely frames Western contemporary animal rights analyses. Both Singer and Regan are notoriously known in feminist circles for their sneering rejection of expressions of "caring" for animals, a position they both offhandedly feminize. Singer prefaces his 1975 book, *Animal Liberation*, a truly groundbreaking work, with a nasty misogynist swipe at a woman who is exuberant in professing her love of animals (while eating a ham sandwich).² Regan prefaces his 1983 treatise, *The Case for Animal Rights*, with an extended complaint against "sentimentality" in animal rights: "all who work on behalf of animals . . . are familiar with the tired charge of being 'irrational,' 'sentimental,' 'emotional,' or worse. . . . We can give the lie to these accusations only by making a concerted effort not to indulge our emotions or parade our sentiments. And that requires making a sustained commitment to rational inquiry" (xii).

Regan and Singer's unflinching embrace of what Susan Bordo (1986) calls "masculinized Cartesian thought" thus paints animal rights, ironically, with the same brush used by those who justify animal exploitation. Josephine Donovan, a prominent feminist animal rights theorist, makes this point: "In their reliance on theory that derives from the mechanistic premises of Enlightenment epistemology (natural rights in the case of Regan and utilitarian calculation in the case of Singer), and in their suppression/denial of emotional knowledge, [they] continue to employ Car-

² Singer's recent rationalization of human-animal sex might also be productively held up to feminist scrutiny.

tesian, or objectivist, modes even while they condemn the scientific practices enabled by them" (1990, 177–78). Evelyn Fox Keller (1978) elaborates on the construction of science as "antithetical to Eros." Sandra Harding (1986) remarks on the synergy between masculinism and appeals to scientific rationality: "Science reaffirms its masculine-dominant practices, and masculine dominance its purportedly objective scientific rationale, through continued mutual support. Not only is this set of associations objectionable because it is sexist; it also makes bad science" (121). The tenor of mainstream animal rights theory set by Regan and Singer taps into this "mutual support" of male reification of Western scientism and rationality, a factor that may largely explain their dominant position in the pantheon of animal rights advocates.

Some of the most exciting work in theorizing a feminist approach to animal rights, then, is rooted in the effort to break from this rationalist tradition. In its place, feminists are developing a new ethic of animal rights around care-based theory. The central philosophical tenets of this approach include consideration of a "particular other" and "attentive love" (a phrase derived from Simone Weil) and a recognition of the importance of feeling, emotions, and personal experience in moral decision making. Several feminists make the point that there is a tradition even among male Western philosophers to locate ethics in emotion, sympathy, and compassion (Hume, Schopenhauer, Buber, Husserl, and Scheler, among others), but that this "sympathy tradition" has been overshadowed by rationalist theory (Donovan 1994).

The best of feminist animal rights theorizing does not simply resuscitate this overshadowed Western philosophical tradition of "care." Rather, it reimagines a human relationship to the nonhuman world by locating action and theory in the lived world and moral universe of women's identity and on the basis of feminist political insights. As a genre, feminist animal rights theorizing thus emerges as one of the sharpest cutting edges of contemporary philosophical and environmental work. Four anthologies encompass the range of this work: a special issue of *Hypatia* edited by Karen Warren in 1991; *Ecofeminism: Women, Animals, Nature*, edited by Greta Gaard (1993); Carol Adams and Josephine Donovan's anthology *Animals and Women: Feminist Theoretical Explorations* (Adams and Donovan 1995); and a second anthology by the same editors, *Beyond Animal Rights: A Feminist Caring Ethic for the Treatment of Animals* (Donovan and Adams 1996).

Some of this work returns us to ecofeminist discourses about women's "special kinship" with animals, an assertion that raises specters of essentialism again. However, most feminist animal rights theorists insist that

developing a care-based ethic cannot rest on an appeal to a "natural(ized)" extension of women's affinities and experiences but rather must also reflect a honed political analysis. Donovan, for example, insists that while a caring ethic might seem to make particular "sense" within women's lives, "feminists must insist that it be framed within a political perspective" (1994, 160), a perspective she enumerates as including analyses of power relations in animal exploitation industries, in the commodification of animals, and in the hegemonic export of Western constructions of human-animal relations.

One of the particular contributions of feminist analyses of these processes lies in exposing the patriarchal foundations of the exploitation of animals and in detailing the gender/race/class specificity of what are typically portrayed as "universal" norms of human-animal relations. The feminist literature on this ranges widely. Carol Adams's (1990) provocative analysis of the masculinist privileging of meat eating, and of feminist interventions to destabilize Western patriarchal (animal) consumption, is a classic. My own brief analysis of the fur industry (in Seager 1993a) adds another dimension to this type of analysis. Feminist analyses of the gendered foundations of industrial and "recreational" animal abuses can be shocking, even in a cultural context where both the casual and the systematic abuse of animals is taken for granted. One of the contributions to this genre that I find particularly revealing and analytically challenging is Jody Emel's analysis of wolf hunting in the United States, "Are You Man Enough, Big and Bad Enough?" (1995). The unremitting ferocity and depravity of wolf eradication in the United States, Emel argues, is deeply embedded in the license given to distorted male power through the normalization of hunting. The hunting and killing of "fierce animals," she argues, is demarcated (in the American frontier-referent imagination) as a pinnacle expression of virility and manhood. In this argument, Emel builds on a rich literature of feminist analyses of hunting (see, e.g., Kheel 1995). But the power of Emel's analysis is in its particularly sophisticated locating of raw male brutality within even larger structures of rationalization: "Constructions of masculinity, cruelty, regimes of bureaucracy, commodity production, class relations, myth and superstition, all determined the wolf's demise. Altogether they supported and mutually defended one another" (1995, 732).

And, finally, a robust area of feminist inquiry interrogates gender, race, and class differences in attitudes to and perceptions of animals, nature, and the environment. A germinal study on American attitudes toward wildlife by Stephen Kellert and Joyce Berry (1987) revealed startling gender differences, with men expressing "dominionistic" and "utilitarian"

attitudes and framing wildlife management priorities in terms of whether animal populations could sustain particular levels of "harvesting," while women expressed more affection-based concern for the welfare of animals and for their protection. Kellert and Berry concluded that gender was the strongest demographic factor in patterning attitudes to wildlife; they went on to point out that to the extent that men dominated the wildlife/conservation bureaucracy and industry, then the formulation of public policies on wildlife would be particularistically gendered. Geographer Jennifer Wolch has extended these analyses with studies of attitudes toward wildlife in California across race, class, and gender (Wolch, Brownlow, and Lassiter 2000; Wolch 2001). She finds, again, sharp differences across gender and also across racial and ethnic groupings in perceptions of animal protection policies, in attitudes toward culturally specific animal practices, and on broad measures of animal welfare.

Public health and feminist environmentalism

Odd as it may seem to any woman who is living with cancer, or who worries about the likelihood of being diagnosed with breast cancer, or who worries about her child's asthma, "health" and "environment" issues until very recently were seldom linked, at least in mainstream and official channels. It has taken (and still takes) relentless pressure from environmental justice and women's health advocates to shift paradigms—to put human health issues on the mainstream environmental movement agenda and to put environmental issues on the health map. Even now, virtually all assertions of causality between health disruptions and environmental assaults are fiercely contested, all the more so when women are the primary proponents of linkage.

Since the 1960s, women's health activists have forged sophisticated transnational coalitions to draw attention to the health needs and threats specific to women. The specificity of those needs and threats are sometimes a consequence of biology (women are "not just small men," as a popular book title proclaims [Goldberg 2002]) and sometimes due to social location, but averring the particularity of *women's* health was the first radical challenge. Beyond drawing attention to "women's issues," these movements simultaneously drew attention to the patriarchal, economic, and social structures that pose particular dangers to women's health and that keep women's health issues from being taken seriously. The recent history of women's health advocacy includes a long list of accomplishments: exposing international patterns of forced sterilization and other systematic reproductive rights abuses, drawing attention to the global epidemic of

violence against women, tracing the emergence of the epidemic of breast cancer in Western countries, challenging the normative assumption of conventional health care and assessment practices that white men's health issues are generalizable and universal, and exposing the dumping of unsafe pharmaceuticals and devices in minority communities and "third world" countries.

On a parallel but separate track, throughout the 1980s and 1990s a growing chorus of voices from women's and social justice movements challenged the mostly male-led mainstream environmental movement on its bias in prioritizing wilderness, animal conservation, and wildlife protection and its concomitant neglect of urban and social environmental issues, including, prominently, human health issues. The human costs of environmental deterioration had always been on the agenda of local, community social justice, and women's groups; indeed, typically, health issues brought many women activists to environmentalism. But mainstream environmental groups were slow—and even resistant—to take up the challenge of human health (Taylor 1989; Bullard 1990; Seager 1993a).

Around the world, public awareness of the impact of environmental deterioration on human health was focused by a series of "discoveries" and spectacular environmental accidents in the 1970s and 1980s: discovery of the ozone hole, chemical disasters in Seveso in 1976 and Bhopal in 1984, nuclear catastrophes at Three Mile Island and Chernobyl among them. But while these hyperevents catalyzed public attention, the attention was usually temporary. The broader trend toward making a sustained connection between health and environmental movements was forged around more mundane and modest health issues that disproportionately affected women and that women were the first to "notice." In the United States and Europe, through sheer persistence and painstaking efforts to develop evidentiary support for their claims, women's health and environmental justice advocates were instrumental in forcing attention to the possible environmental sources of health problems such as escalating rates of asthma and lead poisoning in urban children, epidemics of breast cancer in women in Western countries, and rampant endocrine disruption and "estrogen mimicking" chemical derangement in animals and humans. At the same time, throughout the third world and particularly in South Asia, feminists were collaborating across gender and environmental movements to focus attention on the interlinkages of ecology, health, and "[mal]development."

In both North and South, women's involvement with health/environment issues typically started from concerns about the health of themselves and their families in daily life, an insistence on taking seriously the

particularity of the impacts of environmental degradation on *women's* bodies and health (and particularly reproductive health), and a "personal" connection to "the environment." As Vandana Shiva points out, "Women's involvement in the environmental movement has started with their lives and the severe threat to the health of their families. . . . The 'environment' is not an external, distant category. . . . The 'environment' for women . . . is the place we live in and that means everything that affects our lives" (1994, 2). From women's social location, with their primary responsibility for the health and maintenance of communities and families, "environmental problems become health problems because there is a continuity between the earth and the human body through the processes that maintain life" (Shiva 1994, 3).

One of the most globally ubiquitous threats to human health is the saturation of the environment with "man-made" chemicals—developed and introduced with virtually no understanding of their singular let alone their synergistic impact on humans, animals, and ecosystems. Rachel Carson was not the first to suggest that the chemical fog produced by modern industry was carcinogenic. But her prodigious feat of synthesizing a jumble of scientific and medical information into an understandable, coherent argument about health and environment was transformative. In so doing, Carson pointed the way to the key paths of inquiry for contemporary feminist environmental public health interventions: the importance of persistence in accumulating evidence on chemical/carcinogenic linkages in the face of industry efforts to obfuscate the evidence and to hinder these efforts; foregrounding the synergistic effects of multiple chemical exposures, despite the inherently "unknowable" character of such effects; advocating precaution in the face of scientific uncertainty; and insisting on the necessity of intervening in the fight against cancer at the causal level, not merely the palliative.

At the forty-year mark of the publication of *Silent Spring*, its salience and brilliance remain undiminished. But much of its "information" is out of date. It now seems quaint, for example, to worry as Carson did about "the over 200 basic chemicals [that] have been created for use in killing insects, weeds, rodents and other organisms described in the modern vernacular as 'pests'"; and what a luxury to worry only about "500 new chemicals that annually find their way into actual use in the United States alone" (1962, 7).

Two contemporary books, both of a kinship with *Silent Spring*, are "must" reading for an informational and analytical update on Carson: *Living Downstream* (Steingraber 1997) and *Our Stolen Future* (Colborn, Dumanoski, and Myers 1996). *Our Stolen Future*, a collaboration by sci-

entists and journalists, is the clearest exposition available of the overwhelming evidence that synthetic chemicals are disrupting the hormonal/endocrinal systems of humans and nonhuman animals. Certain broad classes of synthetic chemical compounds, which turn up ubiquitously in consumer products from detergents to paints to plastic food wrap, have the biological effect of "estrogen mimicry" when released into the environment. This raises alarming prospects about several health effects, particularly the "feminization" of male reproductive systems in wildlife (and possibly in humans) and the known association of breast cancer and estrogenic exposure. Knowledge of the relationship between chemicals, endocrine disruption, and cancer is not new; Rachel Carson herself detailed the link in the 1960s. What is new is the evidence pointing to a widening array of substances with these effects, their persistence in the environment, and evidence of the increasing link to breast cancers.

Sandra Steingraber's *Living Downstream* (1997) is a staggering narrative of carefully assembled and interpreted scientific information about the connections between cancer and environmental derangement, interwoven with personal reflections on the author's own struggle with cancer. Like reading Carson, reading Steingraber is maddening and saddening and ultimately inspiring. The title of her book invokes not only a familiar environmental problem but a parable: "this image comes from a fable about a village along a river. The residents who live here, according to parable, begin noticing increasing numbers of drowning people caught in the river's swift current and so went to work inventing ever more elaborate technologies to resuscitate them. So preoccupied were these heroic villagers with rescue and treatment that they never thought to look upstream to see who was pushing the victims in" (xxii).

Steingraber's book is a walk upstream, and her challenge to "business as usual" is uncompromising: "When carcinogens are deliberately or accidentally introduced into the environment, some number of vulnerable persons are consigned to death. . . . These deaths are a form of homicide" (268-69). In a more meditative moment, Steingraber offers a reflection on groundwater pollution and the failure of imagination that produces so much contemporary environmental damage: "Cultivating an ability to imagine these vast basins [of groundwater] beneath us is an imperative need. What is required is a kind of mental divining rod that would connect this subterranean world to the images we see every day: a kettle boiling on the stove, a sprinkler bowing over the garden, a bathtub filling up. Our drinking water should not contain the fear of cancer. The presence of carcinogens in groundwater, no matter how faint, means we have paid too high a price for accepting the unimaginative way things are" (211).

Steingraber's feminist environmentalism is evident in her positionality and her close attention to the carcinogenic subjectivity of the female body. Her position as a woman/scientist/cancer survivor/outsider is explicit. Her engagement with critical environmental analyses and her challenge to the "normal" corporate/industrial capitalist order are explicit. Her resolute rejection of the "lifestyle modification" approaches to preventing breast cancer (so heavily advocated by the medical and cancer care mainstream) puts her in close company with the most radical of the "beyond-the-pink-ribbon" feminist health activists and environmentalists (see, e.g., Brenner and Ehrenreich 2001).

Disappointingly, however, neither in *Living Downstream* nor in *Our Stolen Future* do the authors engage with the rich literature of feminist deconstructions of the institutionalization of gender and power. Both authors are clear that they *are* raising challenges to institutionalized power—confronting the "cancer/chemical industrial complex" and challenging the "medical establishment(s)"—but they do not bring to that challenge feminist analyses of the nature of bureaucracy, of institutions, of the cultures of medical and chemical and regulatory industries. In fact, this is a piece of the puzzle that is missing from most of the feminist environmental health literature. The strength of feminist environmentalism—bringing into focus environmental assaults on the (female) *body*—could be considerably enhanced by curiosity about the production of the culture of (masculinist) institutions that plan and produce those assaults.

Steingraber's latest book, *Having Faith* (2001), is a chronicle of the author's pregnancy that draws on the same stylistic interplay between personal meditation and intensely concentrated environmental analysis. This time Steingraber tracks the assault on the reproductive process, including a detailed assessment of the synthetic chemicals traveling through her pregnant body to the developing fetus. She asks ecological questions of her own pregnancy: How do synthetic chemicals cross the placenta? migrate through amniotic fluid? affect the fetus? Steingraber mostly finds no answers. The absolute lack of curiosity in mainstream science about these questions is itself shocking. *Having Faith* will bring environmentalism to a new readership, but this book has even less structural analysis than *Living Downstream*, and there is a bit too much of the "wonder of life" trope for my tastes. Nonetheless, Steingraber's analysis is worth reading, even when she's a bit sappy.

Rachel Carson died from breast cancer. Surprisingly few people know this. Carson herself was resolutely unforthcoming about her diagnosis (most likely fearing that disclosure of her cancer would be used by her

critics to call into question her integrity as a critic of the chemical industry). But Carson's dual legacy—as one of the most prominent whistleblowers on synthetic chemicals and as a casualty of a “women's” disease that was given short shrift by the male medical establishment—has sparked a remarkable effort to trace the linkages between chemical assaults and breast cancer.

In Carson's name and spirit, feminists who have insisted on bridging the health/environmental gap are transforming what we know about breast cancer in particular and human public health in general. National women's groups such as Breast Cancer Action (www.bcaction.org) and Silent Spring Institute (www.silentspring.org) track medical and environmental advances, releasing a steady stream of analyses and reports on topics as diverse as “Risk of Breast Cancer and Organochloride Exposure,” or “Electromagnetic Exposure as a Potential Risk Factor for Breast Cancer,” or “Awash in Atrazine: Herbicides, Hormones, and Cancer.”⁸

Environmentalism has taken a radical movement—women's health—and made it even more so. It has propelled women's breast cancer support and activist groups well beyond the “pink ribbon” phase (Brenner and Ehrenreich 2001). The new wave of feminist health environmentalism is not a passive enterprise: groups such as Breast Cancer Action do not just “track” information, they are actively involved in the production of new knowledge. One of the central insights of community-based, social justice, and feminist environmental organizing is that the human costs of environmental destruction accrue differently across sexes, races, classes, ethnicities, and geographies. One repeated pattern manifested at scales from the local to the global is that the health “fallout” from environmental damage cascades down the social power gradient: people marginalized or stigmatized, people without a voice in the official expert and authority structures, people on the economic and social edges, feel the effects of environmental derangement first, longest, and most acutely. A related insight is that “place matters.” Following these analytical bread crumbs, feminist environmentalists are on the leading edge of developing and deploying spatial proximity analysis.

Social justice environmentalism, including women's local activism on health and environment, has often been prompted by a local awareness of unusual distributions and odd geographic “clusters” of illness or of environmentally dangerous facilities and activities. In the last five or six years, activists have transformed this awareness into a powerful tool of

⁸ These and related reports are available at: www.ourstolenfuture.org; www.silentspring.org; www.bcaction.org.

environmental investigation. Using spatial analysis and particularly geographic information system (GIS) technologies, justice and health activists have started to develop sophisticated locational analyses of environmental correlations. Use of GIS allows the simultaneous mapping of layers of demographic, environmental, land use, historical, and social data, offering the ability to transpose maps of breast cancer incidence, for example, over maps of known chemical release sites. There is virtually no limit to the number of layers of data that can be accumulated and compared. Currently, two of the largest environmental health studies in the United States involve multidisciplinary research teams working with women's environmental health grassroots organizations in Massachusetts and New York to assess the elevated rates and distinctive geographic patterning of disproportionately high rates of breast cancer incidence found on Cape Cod and Long Island.⁴

This is a breakthrough adaptation of technology for feminist inquiry. In the environmental community, women's breast cancer researchers and activists have been in the lead in the adaptation of these technologies, and their efforts are already changing the broader nature of environmental investigation. Adoption (and adaptation) of this "scientific" approach has won for women and women's issues a new respect. Women's complaints about breast cancer (and other illness) clusters—previously dismissed as phantoms of overwrought female anxiety—are now being taken seriously.

But this engagement with mainstream science needs careful scrutiny. This is a good moment in feminist environmentalism to revisit Audre Lorde's caution about the use of the master's tools to dismantle the master's house. By entering into the environmental "big time" through the portal of mainline science, feminists risk losing their distinctive stance of oppositionality and their insistence that environmental knowledge comes in myriad forms. They also risk losing their way in what Sandra Steingraber has called the "miasma of uncertainty" (1997, 71). Breast cancer researchers using GIS technologies are entering into the "scientific proof" game on terms that they do not set, and on terms that are stacked against them.

Environmental investigation of all kinds is dogged by "scientific uncertainty"—the fact that it is almost impossible to "prove" direct environmental causes and effects. Ecological systems from the local to the global are complex and not fully comprehended by tools of scientific

⁴ Health/GIS studies are also occurring elsewhere, but the Massachusetts and New York breast cancer studies are the largest. Information on these studies is available through www.silent Spring.org and www.healthgis-li.com.

investigation; in the environmental domain, there is almost always room for (scientific) doubt, and the possibility of contravening evidence is always present. This uncertainty is particularly acute in matters of health: it is virtually impossible to establish scientifically certain "proof" that exposure to any given chemical at any given point in time "causes" cancer that may appear several years later and in people who may have moved to a new location far from the exposure site.

Scientific uncertainty serves as a refuge for scoundrels of all kinds. Chemical-producing and pollution-causing industries have relied for years on the "cover" that scientific uncertainty affords them. The assurance that there will always be scientific uncertainty has long given solace to polluters and has served as a barrier to enactment of remediative public policy and legislation. The uncertainty that is inherent in environmental assessment favors policy inaction, particularly in the hands of politically conservative politicians and regulators.

However, that paradigm is about to shift. In the last decade, the articulation of a principle of "precaution" is one of the most radical developments in global environmental thought. The most far-reaching implication of the "precautionary principle" may be that it offers an intervention against the closed loop of scientific uncertainty. The commonsensical and deceptively simple precautionary principle displaces the expectation that environmental action requires post facto scientific proof of harm. In a series of interlinked doctrines, the precautionary principle asserts that public and private interests have a positive obligation to act to prevent environmental/health harm before it occurs; that the indication of harm, rather than "proof" of harm, should be the trigger for action; that the burden of proof needs to be shifted to the front of the chain of production (the presumption of safety should be tested before potentially harmful substances are released into the environment rather than waiting to test for harm after the fact); and that all activities with potential health consequences should be guided by the principle of the least toxic alternative.⁵

A 1998 conference in Wingspread, Illinois, brought together environmental activists, scientists, and ethicists to formalize and gather into a single document the strands of precautionary environmental thought that have been emerging over the last half century. Several feminist environmentalists and health activists participated in drafting the "Wingspread

⁵ The precautionary principle is now widely discussed. Some of the best articulations of it can be found in Stringer 1997, Raffensperger and Tickner 1999; and the Web site of the Science and Environmental Health Network, www.sehn.org.

Statement on the Precautionary Principle" (1998), and the influence of their social justice perspective is evident in the final document, which reads in part: "The release and use of toxic substances, the exploitation of resources, and physical alterations of the environment have had substantial unintended consequences affecting human health and the environment. Some of these concerns are high rates of learning deficiencies, asthma, cancer, birth defects and species extinctions. . . . Therefore it is necessary to implement the Precautionary Principle: When an activity raises threats of harm to human health or the environment, precautionary measures should be taken even if some cause and effect relationships are not fully established scientifically."

The Wingspread statement built on the work of countless antecedents and forebears. Prominent among others, Rachel Carson pressed the case for preventing carcinogens from being developed and released rather than waiting until the damage was done and then trying to find a "cure." Elements of a precautionary principle shaped a 1987 European treaty that banned the dumping of toxic substances into the North Sea. The precautionary principle also echoes ethical positions that social justice and health activists—women prominently among them—have articulated for years. Integral to the precautionary principle is an ethical underpinning that Carolyn Raffensperger calls "forecaring" (Raffensperger 2000). As with the emergence of women's animal rights theorizing, this positionality of "caring for" and "forecaring" is distinctly feminized and—at least nascently—feminist.

And now it has policy clout. An overhaul of environmental laws is under way in Sweden and Germany to bring regulation and practice into compliance with precaution. As I write this in early 2002, the European Union is seriously studying a Union-wide policy shift to bring industrial and environmental policies into alignment with precautionary principles. While the regime of scientific uncertainty is not poised to fall quite yet, hairline cracks are showing in its foundation.

Global political economy and feminist environmentalism

Feminist environmentalism digs deepest into structural explanations for the state of the earth in work done under the broad rubric of "feminist political ecology." This is not a coherent subfield in a disciplinary sense, but rather it describes practices and inquiries at a point of convergence of critical studies of science, global structural power, gender, and environment. Work in this domain ranges widely; much of it focuses on transnational or international processes, and most feminist political ecology

starts from a curiosity about the material conditions of lives rooted in specific environmental contexts. Much of this work includes a strong focus on the uneven distribution of access to and control over resources, and economics of uneven development (see, e.g., Rocheleau, Thomas-Slayter, and Wangari 1996; Sachs 1996); challenges to modernist inscriptions of resource-as-commodity relationships, especially the imposition of Western systems of the commodification of nature; and exposing environmental effects of the forced integration of local environments/communities into global capital flows, world trade regimes, and military webs.⁶

Gender is a particularly important explanatory variable in environmental relations of these kinds. Several axes of power define people's access to resources and to control over environments, but the particular foregrounding of gender, as well as race and class, across scales from the local to the global, produces potent analyses. The gendered nature of ecologically based power structures is often most apparent at moments of ecological change: when land is transferred from a commons system to private ownership, for example, or from forest to cropland, or from subsistence cropping to export cash cropping. Such shifts not only reveal patterns of gendered power but are also instrumental in the actual transference of power—typically away from women to men and away from local control to external control.

Feminist scholarship on the construction of science, much of which is built on the foundation set by Carolyn Merchant (1980), exerts a strong influence on the political ecology literature. The widespread export of Western technologies and ideologies of the "control of nature," whether as part of eighteenth-century colonialism or twenty-first century corporatist "development" strategies, is a strong determinant of the state of the global environment, and feminist political ecologists have been particularly attentive to the gendered import of the global spread of these ideologies (see, e.g., Shiva 1988, 1993, 2000; Agarwal 1992; Seager 1993b).

Blaming women as environmental policy: The population debates

One of the issues that has driven feminist political ecology for the past decade—and promises to continue to be a front-burner issue—is the "population question." This is one of the most contentious domains in environmental explanation and is the issue where feminist positionalities are

⁶ Sardar 1988; Agarwal 1992; Mies and Shiva 1993; Seager 1999; Sulliman and King 1999; Kurian 2000, Shiva 2000.

in sharpest contradistinction to (and sharpest conflict with) mainstream environmentalism (Hartmann 1994; Bandarage 1997).

The "overpopulation" explanation for global environmental ills just will not go away, despite compelling evidence that points to other forces of environmental derangement, particularly at the global scale. Alternative analyses spotlight larger structures of inequality and manipulation, such as the maldistribution of social (and natural) resources at global and local scales, the hyperconsumption of affluence, resource extraction pressures exerted by globalization and global flows of capital, presumptions of the "imperatives" of constant economic growth, bioprospecting by the first world in the third, agroindustrialization, the largely unregulated production and disposal of toxic chemicals and "by-products" of industrial processes, and rampant global militarism that destroys vast swaths of the earth, consumes resources at an unimaginable rate, and distorts budgets and national priorities everywhere.

Nonetheless, mainstream environmental analysis—both in the popular press and imagination and in the official policy positions of many of the major U.S. and European environmental groups—detours neatly around these structural factors to place the blame for global environmental ills, instead, on the fertility of women in the third world. Population "problems" are associated with faceless and undifferentiated poor women of color in intricately coded ways. At the same time, actual women's lives are left entirely out of the environmental picture. Blaming women without ever actually paying attention to them is a standard patriarchal analytical feat, but it seems particularly pernicious in population environment debates.

Poor women, "minority" women, and women in non-Euro-American countries pay a high price for being blamed for global environmental ills. Population control—in many guises, including anti-immigrant political alliances between environmentalists and nativist groups—is a particularly harmful "green" solution for women. Unbridled racism and sexism are intertwined with the politics of international fertility control; if nothing else, this makes its advocacy by predominantly white, male, first-world environmental groups particularly disturbing. Environmental advocates of population control typically ignore its history and ideological underpinnings, especially its association with repressive regimes and genocidal governments. Mainstream environmentalists are uncomfortable acknowledging that population control, no matter how euphemistically couched, is essentially a vehicle for the control of women; intervening in "fertility" always means, above all, intervening in women's lives, in female repro-

ductive organs, and in the exercise of reproductive freedom. Population control always implies the exercise of centralized authority—a government, typically in concert with international development agencies—in imposing restrictions on women’s reproductive activities. In the name of population control, women are used as international guinea pigs for birth control wonders produced by pharmaceutical conglomerates in the rich world; third-world women’s bodies are dumping grounds for medications that first-world industry can no longer sell at home; women have been subjected to mass sterilizations, without consent, in Puerto Rico, India, China, Peru, and on Native American reservations.

Feminist environmentalists are active in producing a critical literature opposing the prevailing populationism (see, e.g., Hartmann 1994; Bandarage 1997; Silliman and King 1999). In the early 1990s, an international group of feminist scholars and activists created an alliance, the Committee on Women, Population and Environment (CWPE), to establish an environmental position to counter “populationism.”⁷ The CWPE’s 1992 “Statement on Women, Population and Environment” (reprinted in Silliman and King 1999) made some inroads into the environmental consensus at the 1992 United Nations environmental conference in Rio de Janeiro, and CWPE continues to exert influence in international environmental circles, but population-based environmental explanation is deeply entrenched and mostly unmoved by these countervailing feminist positions. As recently as 2001, for example, John Flicker, president of the (U.S.) National Audubon Society, unequivocally asserted that “human population growth is the most pressing environmental problem facing the U.S. and the world.” In 1999, a popular book from the influential U.S.-based Worldwatch Institute used odd metaphors to drive home its anxiety about (over)population: “As the global population locomotive hurtles forward—despite pressure applied to the demographic brakes—there are hazards on the tracks ahead” (Brown, Gardner, and Halweil 1999, 22); and later, “What is needed, to use a basketball term, is a full-court press—an all-out effort to lower fertility, particularly in high-fertility countries, before demographic fatigue takes over” (127). More worryingly, government policies in several key first-world states explicitly identify population as a preeminent global environmental problem.

Given the precarious state of the earth—and the urgent need to come to terms with the real causes of environmental destruction—the fierceness with which many environmentalists cling to populationist explanations

⁷ The CWPE can be reached through the Population and Development program, Hampshire College, Amherst, MA 01002; it produces a quarterly publication, *Political Environments*.

itself needs explanation. At its core, the attachment to populationism is a deeply patriarchal obsession: in the patriarchal worldview, women's bodies are seen to be malleable objects of public policy intervention. In the global environmental arena, it is presumed to be easier, cheaper, and faster to interfere in women's fertility than to challenge large masculinist structures such as militarism and global capital formation; and, indeed, in a patriarchal global culture it is easier, cheaper, and faster to do so. But blaming women for the sorry state of the earth is shabby policy and bad analysis—and it will not solve environmental problems either.

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Book Reviews

Rosalind Franklin: The Dark Lady of DNA. By Brenda Maddox. New York: HarperCollins, 2002.

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I assume everyone who comes to this book has heard about DNA and that it forms a double helix; probably also about Watson and Crick, and, if a feminist, about Rosalind Franklin, the scientist who was cheated of her share of the credit for elucidating its structure. So, let me just quickly review how we know this.

In 1968 came *The Double Helix*, James Watson's best-selling memoir of the Scientist as a Young Man, chasing girls, and the structure of DNA.¹ While he and Francis Crick were pursuing their quarry at Cambridge University in the early 1950s, at King's College London their hapless friend Maurice Wilkins was crossed in his parallel search by the wicked witch "Rosy," who was intent to go at DNA on her own. So, what could Watson and Crick do but win their undeclared race using "Rosy's" data behind her back? Of course, that way they cut Wilkins, too, to the chase, but it all ended well because, in 1962, the Nobel Committee lauded all three. By then, the wicked witch was out of the picture, having died of cancer at age thirty-seven.

Next, in 1975, came *Rosalind Franklin and DNA*, a cri de coeur by Franklin's friend Anne Sayre.² Starting to read *The Double Helix*, Sayre immediately realized that Watson's "Rosy" bore no likeness to her friend Rosalind physically or otherwise. And not even her intimates ever called her Rosy. So, what made Watson invent "Rosy"? The answer was not hard to find. If Watson were to tell all, he would have had to admit that he and Crick got hold of Franklin's data without her knowledge or consent. But what if he made her not a scientist but Wilkins's uncooperative tech-

¹ James D. Watson, *The Double Helix* (New York: Atheneum, 1968).

² Anne Sayre, *Rosalind Franklin and DNA* (New York: W. W. Norton, 1975).

nician whose angry outbursts threatened to stem the progress of science? *Et voilà*, "Rosy," and a different story indeed. Sayre exposed this story, but her book, of course, never reached the large audience that flocked to Watson's tale of how science can be fun and win Nobels too.

So, the most anyone but Franklin's close friends and colleagues have known about her is that, between 1951 and 1953, she spent an unhappy time at King's, where she did groundbreaking experiments on DNA and then moved to Birkbeck College London, where she and her colleagues explored the structure of viruses until her untimely death in 1958. They may also know that Watson and Crick appropriated Franklin's work on DNA and, in 1962, shared with Wilkins the Nobel Prize for establishing its structure. But that is the stuff of tragedy, not of a life.

Now, fifty years after the King's/Cambridge drama, Brenda Maddox has made this remarkable woman come to life, showing her to have been a talented, handsome, elegant, and sophisticated scientist and intellectual; an adventurous traveler, sportswoman, and mountain climber; the scion of a large, distinguished Anglo-Jewish family whose forebears came to England in the 1740s and 1750s. No brawling "Rosy," she. If colleagues and friends at times felt she expected too much from them and from herself, that came from her family's long legacy of high standards toward work, social relationships, public service, and philanthropy.

Maddox does not dwell excessively on the saga of the double helix; it is an old story that many people know. Instead she gives us a vivid portrait of a daughter, friend, and scientist whose tragedy was that she died when her work was at its peak and she, at long last, could look forward to stable funding and institutional support for herself and the research team she was leading at Birkbeck College at the time of her death. She "knew her worth. With every prospect of going on to further significant achievement . . . she was cheated of the only thing she really wanted: the chance to complete her work. The lost prize was life" (328).

Maddox points out many ironies. For example, in 2000 King's College London, apparently in an effort to right the wrongs of a half century before, dedicated a new building named the Franklin-Wilkins Building. Thus King's linked the names of Maurice Wilkins, Nobel Laureate and for nearly fifty years a professor at King's, with that of Rosalind Franklin, who had been forced out in 1953. On this occasion Watson appeared in person to say that Franklin's "contribution" had been "critical" to his and Crick's "discovery" (323).

Maddox seems to have talked to everyone who ever knew Franklin and to have read everything Franklin wrote from when she first learned to

write. This is a superb biography, beautifully written. Read it, assign it to your students, and give it to your colleagues and friends.

Maddox's prologue ends with a description of Franklin that could well stand as her epitaph: "She achieved an international reputation in three different fields of scientific research while at the same time nourishing a passion for travel, a gift for friendship, a love of clothes and good food and a strong political conscience. She never flagged in her duties to the Anglo-Jewish family of which she was a loyal, if combative, member. . . . The measure of her success lies in the strength of her friendships, the devotion of her colleagues, the vitality of her letters and a legacy of discovery that would do credit to a scientific career twice its length" (xviii-xix). 1

Cybersexualities: A Reader on Feminist Theory, Cyborgs and Cyberspace.
Edited by Jenny Wolmark. Edinburgh: Edinburgh University Press, 1999.

Global Obscenities: Patriarchy, Capitalism, and the Lure of Cyberfantasy.
By Zillah Eisenstein. New York and London: New York University Press, 1998.

Christine Wertheim, Institute for Figuring, Los Angeles

Margaret Wertheim, California Institute of the Arts, Los Angeles

If "woman" is not a singular category primarily defined by her difference from man but is internally divided, both within herself and from others of her sex—according to geographical, political, economic, class, and race position, and her psychosexual orientation—can we imagine this diversity in ways that will enhance the political and material actuality of women's lives today? If so, can science and technology help us in the task? Though differing in their approaches, both Jenny Wolmark and Zillah Eisenstein answer these questions affirmatively, supporting feminisms of difference by focusing on the impact of new technologies and their fictional incarnations on the politics of gender.

Wolmark's *Reader* surveys the subdiscipline of "cyberfeminism" that has grown in the last two decades around feminism's encounter with cyberspace and specifically around Donna Haraway's groundbreaking essay "A Manifesto for Cyborgs: Science, Technology, and Socialist Femi-

nisms in the 1980s.”¹ Comprehensively introduced by Wolmark, the book is divided into three sections, the first dealing with issues of embodiment and the metaphor of cyberspace, the second with cyberpunk literature and the metaphor of the cyborg as a model for nonunitary forms of subjectivity. In the final, more diffuse section, category boundaries begin to blur, with contributors here arguing for what Wolmark calls “a more creative account of difference in postmodern, cybernetic theory” (241).

The first section is predominantly concerned with the way both anxieties and pleasures surrounding technology are “frequently displaced onto the figure of the woman” (14). Many of these essays deal specifically with films. In academic discourse the concept of cyborg embodiment is frequently entwined with heated debates over sexuality and gender roles; in popular narratives, however, as Wolmark notes and many of these essays make clear, there is “a singular unwillingness to engage” in such issues (15). Mary Anne Doane, for example, drawing on Kristevan abjection to examine the maternal in *Alien/s* and *Blade Runner*, and Claudia Springer, writing on the eroticization of technology in *Terminator* and *Robocop*, argue that the popular masculine imaginary conflates its fears of technology with the “castration anxiety” aroused by the sight of the female body. Whether this results in the confusion of identities inherent in the very concept of motherhood (Doane) or an eroticization of the man-machine interface (Springer), contemporary accounts of technology have been infected by men’s postwar (World Wars I and II) awareness of their bodies and hence their egos’ vulnerability.

Sadie Plant argues that women are indeed like computers, because they can “mimic any function” (112). Beginning with an extended reflection on Ada Lovelace—history’s first computer programmer—Plant draws on Luce Irigaray to argue that women have woven themselves into the fabric of history not by asserting some essential story of their own but through appropriation and simulation. However, this unknown and unknowable nonessentiality does not denote their erasure, Plant says; rather, it ensures that they, like the machines, are already adapted to the (cyber)future in which individualized bodies and identities will no longer be required—a view that, Wolmark notes in her well-balanced introduction, may “depend on precisely the kind of technological fetishism of which Mary Anne Doane was so critical in her essay” (18).

Arguably the finest essay in this volume is Elizabeth Grosz’s “Space, Time and Bodies” which, though not specifically a discussion of embod-

¹ Donna Haraway, “A Manifesto for Cyborgs: Science, Technology, and Socialist Feminism in the 1980s,” *Socialist Review* 15 (March/April 1985): 64–107.

iment from the perspective of cybertheory, raises critical questions about the ways in which women's experience has been continually excluded from Western thinking about the very concepts of space and time. Grosz's essay has rightly been included in this volume, for, to quote Wolmark, cybertheory has too often "underestimated the complex interrelationship between theoretical notions of space and time and representations of subjectivity" (19). Analyses of cyberspace, while rightly pointing to the possibilities inherent in post-Euclidean/post-Newtonian conceptions of space with which modern technologies are seen to be allied, fail to notice that these very conceptions often continue to affirm what Grosz calls "the fundamental masculinity of the knower" (134). Changes in our conceptions or practices of space do not in and of themselves signify changes in the social possibilities for new subjective structurings if the experiences they allow and describe continue to exclude half the human race.

In the book's second section the focus turns to the figure of the cyborg in cyberpunk novels, hailed by Wolmark as the "quintessentially post-modern" literary practice (139). With the notable exception of N. Katherine Hayles's illuminating exploration of the ways cyborg narrative structures are used as tools for (re)conceptualizing different phases of the psychosexual life cycle, these essays tend to exhibit the same pathology as the literature they detail, taking the cyborg metaphor too literally. Here, Haraway's influence is especially evident; for her, "woman" is not a category but the living embodiment of the struggle to hold contradictory elements together, at the level of both organism and sign. The "cyborg" is a metaphor for this dynamic—it names the condition of women's lives within the logic of late capitalism. If we (women) are all cyborgs, Haraway argues, this is not because we have physically or psychically incorporated the products of technoscience but because we fail to conform to current taxonomic structures. The archetype of the cyborg in her schema is not a body infused with technology but the woman of color. There is much to be said for Haraway's analysis, which originally had an ironic dimension; by literalizing this metaphor might cyberfeminism itself be in danger of inventing a new essentialism?

That is the question raised in the final section of Wolmark's diverse reader. Here, a variety of authors argue that the cyborg is predominantly a metaphor not for individual bodies but for political coalitions. As these essays suggest, feminisms of difference call for a politics that can contain disparate and even conflicting ideals: dissensus and partiality, not democracy and totality. Wolmark uses the notion of "inappropriate/d others," which Haraway borrowed from Vietnamese-American filmmaker and theorist Trinh Minh-ha, to frame this most political of Wolmark's three sec-

tions. For Haraway, to be(come) inappropriate/d “means to be in a critical, deconstructive relationality . . . a diffracting rather than reflecting (ratio)nality” that exceeds current structures of domination and power (320). She uses this idea to argue for a diffracted feminist allegory that might have the power to move us beyond patriarchal narratives of enlightened, disembodied subjectivity to a “world called elsewhere” in which current orthodoxies of race, class, age, and sexuality are undone (320). In summary, Wolmark’s reader stresses that the politics of gender is itself infused with metaphor and narrative. If women have not so far made history, they may come, through creative appropriation of technological metaphors and the generous realization of radical differences, to weave some future herstories.

But while computer-mediated activity may contribute to the reconstruction of women’s (self-)image, the cyber-media-corporate complex also uses its promissory potential to obscure its own role in delimiting its democratic capacity. In *Global Obscenities: Patriarchy, Capitalism, and the Lure of Cyberfantasy*, Zillah Eisenstein sets out to unmask “the power relations in cyberspace” (71). Less speculative and theoretical than Wolmark’s reader, Eisenstein’s lucid analysis is formed around the factual datum of the global cybereconomy, which even a cursory glance reveals as appallingly inequitable: “Eighty four percent of computer users are found in north america and northern europe. Sixty nine percent are male . . . with an average household income of \$59,000,” and only 3 percent of African Americans subscribe to on-line services (73). Fully two-thirds of the world’s people have never made a phone call! Under such conditions, Eisenstein argues, cyberdiscourse fashions an imaginary vision that if powerful for some must be seen as only part of the picture. Or as we might ask vis-à-vis the World Wide Web: Whose world are we talking about? I

Lessons from the Damned: Queers, Whores, and Junkies Respond to AIDS. By Nancy E. Stoller. New York and London: Routledge, 1998.

How to Have Theory in an Epidemic: Cultural Chronicles of AIDS. By Paula A. Treichler. Durham, N.C., and London: Duke University Press, 1999.

Women Take Care: Gender, Race, and the Culture of AIDS. By Katie Hogan. Ithaca, N.Y., and London: Cornell University Press, 2001.

Sheila McManus, University of Winnipeg

If it seems hard to believe that we are at the beginning of the third decade of the AIDS epidemic, these books serve to remind us of how far we have come, and how very far we still have to go, in the fight against AIDS. Taken together, they cover most of the last twenty years of the history of AIDS in the United States and overlap in some provocative ways.

Nancy Stoller's 1998 book *Lessons from the Damned* is grounded in her experiences as an activist with various AIDS organizations over the years. The book discusses the birth and growth of several San Francisco-based organizations as well as ACT UP New York. Far from simply relating a tale of activism in these two cities, however, Stoller analyzes what worked, what didn't, and what had to change along the way, and thus offers lessons to AIDS organizations across the United States and around the globe. Her goal is to document "the way that poor people, people of color, gay men and lesbians, drug users, and women have built a social movement to oppose AIDS' devastating impact," as well as "the ways that racism, sexism, and class have both limited and energized the responses of community organizations to the epidemic" (1). She profiles groups as diverse as the now-mainstream San Francisco AIDS Foundation, the "marginally legal" (2) needle exchange group Prevention Point, and the California Prevention Education Project (originally called the Prostitutes' Education Project), all of which had to confront similar issues of inclusivity as they grew and developed.

Stoller is fair and even-handed, crediting each organization for its very real accomplishments serving the needs of its community, while critiquing the choices and compromises each had to make to secure more funding, more credibility, or a broader base of political support. Her analysis of the strengths and weaknesses of a range of different political and organizational models is particularly useful, and this information could be ap-

plied by other groups that are involved in AIDS activism. For example, a key stumbling block for all of the organizations she examines was their inability to work effectively across multiple lines of race, class, ethnicity, gender, and sexuality. The unparalleled ability of ACT UP to get the media's attention, and thus shape the tone and content of AIDS discourse, was rooted in the privilege, resources, and aesthetics of white, educated, urban, gay males; its campaigns aimed at other communities were never as colorful or effective. This fact does not take anything away from ACT UP's successes, but it does help to explain why it succeeded in some areas and failed in others. New and established AIDS organizations could learn much from this book.

Given Stoller's focus on frontline activism, one might not expect *Lessons from the Damned* and *How to Have Theory in an Epidemic* to have much to say to each other, yet they share a good deal of common ground. Paula Treichler's 1999 study traces what she calls the "epidemic of signification" that has paralleled and fundamentally shaped the AIDS epidemic, the abundance of "meanings, definitions, and attributions" that challenge the efforts of scientists and doctors to insist that it is "an epidemic of infectious disease and nothing more" (1). What we think we know about AIDS, what we can say and how we can say it, has always already been shaped by the words, labels, and categories of this epidemic of signification, a point she ably demonstrates through her close reading of multiple "cultural chronicles" of AIDS.

Treichler's study covers an enormous amount of material. For example, she traces the complicated relationship between the current "common sense truth" that a person is more likely to become infected through the "vulnerable anus" than the "rugged vagina" to an inaccurate and wildly heterosexist 1985 article in *Discover* by John Langone (17). The book also includes no less than two chapters on AIDS and HIV infection in Africa and the "third world." In one of these chapters she discusses a 1990 *New York Times* series about AIDS in Africa, noting that while it did represent a significant improvement over earlier articles, it was unable to escape a colonial perspective. It cited five "co-factors" for the rapid spread of "heterosexual AIDS" in Africa: "rampant sexually-transmitted diseases," "lack of male circumcision," "little-known sexual practices," "promiscuity," and of course "prostitution" (207), demonstrating that what the developed world thinks it "knows" about AIDS in Africa has more to do with racist Western stereotypes about Africa than with any kind of African reality. *How to Have Theory in an Epidemic* makes it plain that the "cultural evolution" of AIDS has not yet managed to move

beyond a depressingly familiar terrain of racism, sexism, classism, homophobia, and colonialism.

Katie Hogan's 2001 *Women Take Care* examines in detail one aspect of this terrain—the discourse of maternalism and the image of the “good woman” as caretaker. Where Stoller focuses on the stories of activists and Treichler's strength is with visual texts, Hogan brings a close reading of fiction, nonfiction, and film to her argument and grounds it in earlier American narratives of maternalism and caretaking in the face of social problems. Films such as *Boys on the Side* (1995) and *Martin's Room* (1997), and novels such as Harriet Beecher Stowe's *Uncle Tom's Cabin* (1852), Perri Klass's *Other Women's Children* (1990), and Pearl Cleage's *What Looks Like Crazy on an Ordinary Day* (1997) are analyzed in terms of their representation and normalization of women's self-effacing caregiving.

Hogan argues that “literary and visual responses to the pandemic” do break the silence “on women and AIDS” but do so through narratives that “make the pandemic acceptable to white heterosexual Americans” (xi). For example, the character Jane in *Boys on the Side* is black and a lesbian but escapes this marginalized identity by becoming the desexualized mammy caretaker of a classic and more acceptable AIDS “victim”—a white woman who contracted HIV through heterosexual intercourse. The figure of the “good woman,” temporarily redrawn to include a black lesbian, normalizes the harsher realities of AIDS and American society, such as sexuality, racism, and poverty, while reinscribing the high cultural value placed on women who put other people's needs before their own. The visibility “women” gain through this palatable image of the “good woman/caretaker” thus comes at a high price: “it silences the lives of flesh-and-blood women and distorts the realities and struggles of those who have died” (ix).

Hogan ends her book by discussing Tiye Milan Selah's 1994 short story “An Elegy for Jade,” which “addresses many of the real problems black women with HIV and AIDS encounter—invisibility, ignorance, and lack of care linked to historically entrenched racism and sexism. It also forcefully abandons the idealized sacrificial woman as solution and device” (130). Choosing to close the study with a text that rejects the “good woman/caretaker” provides a strong endnote for her argument, but the conclusion as a whole needs to connect her analysis of that text with a key point she made in her introduction, namely, that Western culture must stop gendering caregiving as feminine and natural and private (6). A deeply rooted reliance on the discourse of the good woman/caretaker is proving

deadly to women in the face of the AIDS epidemic, but its long and complex history means that challenging it will not be easy and will require a range of ideas and suggestions.

Hogan's conclusion aside, all three of these books have important lessons to teach, and the remarkable amount of cultural and political territory they cover makes two of the points at which they meet all the more powerful. First, it is painfully obvious that race, class, gender, and sexuality continue to inform every facet of the epidemic, from the ways we comprehend it to the ways we fight it, and women from all classes, races, and sexualities have suffered as a result. Treichler notes that selected gynecological conditions were not added to the clinical profile of HIV/AIDS until 1992 (53). Stoller points out that the First National Scientific Meeting on HIV Infection in Adult and Adolescent Women was not held until 1995 (10). Hogan adds that women with AIDS still tend to disappear behind such categories as "pediatric AIDS" because children's health is considered to be more important than women's health and there is greater cultural value, dignity, and sympathy available to "good mothers" than to women with socially unacceptable diseases.

Second, and closely related to the first, all three books make it plain that mainstream feminism dropped the ball during the first two decades of the epidemic. As Treichler states, feminism "failed to influence the direction of the epidemic or challenge the stereotypes and misconceptions that have pervaded AIDS discourse thus far" (236). In the last ten years AIDS has become one of the leading causes of death among women in the United States, and yet it has not become a staple topic in feminist writing or activism.

Luckily, these works offer three clear strategies for the future. First, we must continue to challenge the existing categories of race, class, gender, and sexuality that have shaped the epidemic itself and our responses to it. As Stoller writes, fewer assumptions about "other" communities and more "group interaction and communication" (137) are needed and should be put in place by AIDS organizations from the very beginning. Second, mainstream feminism must bring its years of activism, analysis, strategic savvy, and loud voices more fully into the fight against AIDS because it is not just "other women" who get AIDS. And, finally, we must continue to ask questions. The point of theory in an epidemic is to force us to be skeptical, to question what lies behind what we are told and what we believe to be true, to continually ask questions in the hope that our answers get better. The daily battles for survival, for funding, and for hope will and must go on. If you are not yet concerned about AIDS, these books will convince you that you should be, and they suggest that the

lessons learned during the first two decades of AIDS could make the third decade the last. 1

Breast Cancer: Society Shapes an Epidemic. Edited by Anne S. Kasper and Susan J. Ferguson. New York: St. Martin's Press, 2000.

Manmade Breast Cancers. By Zillah Eisenstein. Ithaca, N.Y.: Cornell University Press, 2001.

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Silence around breast cancer is no longer the major problem it was when Audre Lorde published her groundbreaking *The Cancer Journals* in 1980. Today's problems include media overload bordering on hype; lack of detailed analysis in the mainstream media of the scientific evidence behind (or absent from) detection, diagnosis, and treatment of breast cancer; and a political understanding of breast cancer as a disease beyond the individual, beyond finding the best doctors, the best support systems, and the most promising scientific research. These two books exemplify the current feminist effort to understand breast cancer not only as a disease until recently shrouded in silence and neglect because of sexist bias but also as a site of struggle at the intersections of "science" and "politics," the very intersections long denied within traditional science.

The radical women's health movement in the United States of the 1970s identified significant issues (among them the devastation wrought by—and lack of evidence to support—radical mastectomy as the standard treatment for breast cancer, as well as the absence of informed consent in breast cancer surgery) that continue to include conflicts over the very definitions of health care, of science, and of the role of grassroots activism in the biomedical system. Both books map the story that, as Anne Kasper and Susan Ferguson state, "has not yet been told [about the] powerful ways that the social forces that we take for granted have deeply influenced and forged women's experiences of breast cancer" (v). Further, as Kasper and Ferguson make clear in their preface, the books "critically examine these social forces and clarify how research science, the health care system, the economy, and the media . . . make breast cancer more than just another disease to be treated" (v). Both texts acknowledge their debt to the radical women's health movement in the United States, and both are

valuable guides to the radical re-visioning required to eliminate the epidemic of breast cancer in this country.

Kasper and Ferguson have brought together eleven essays that build the arguments for understanding breast cancer as a socially constructed disease. They also include a useful foreword by breast surgeon and researcher Susan Love on the origins of the current advocacy movement and their own conclusion spelling out a radical agenda for change. This excellent book is organized into three parts: the historical context of diagnoses and treatment; breast cancer as a social problem of economics, politics, and women's bodies; and reflections on social change.

In part 1, on the historical context of diagnoses and treatment, historian and physician Barron Lerner traces how the medical profession transformed breast cancer from an incurable to a curable disease—simply by saying that a woman who is alive five years after treatment is “cured.” Defining a breast cancer “survivor” in arbitrary terms that had no scientific basis was a tragically misleading position that has only recently been addressed by the medical profession and quietly changed.

In another essay on historical context, coeditor Ferguson, a sociologist at Grinnell College, tells the gruesome story of how women's breasts joined the list of body characteristics (such as weight) transformed ideologically and materially into medically controllable “problems.” Plastic surgeons invented an insurance-reimbursable solution to extremely small breasts by coining the illness “hypoplasia,” illustrating one way that the current economic structure of the U.S. health care system figures in the medicalization of women's bodies. Ferguson's valuable historical analysis of the medicalization of women's breasts places decision making about the kinds of surgery and options for reconstruction after cancer treatment into the larger context of breast augmentation, literally shaped by the American Medical Association, the plastic surgeons' professional organization, and the companies that manufacture breast implants. In addition to helping us understand the current medicalized perspective on breast cancer and the vagaries of our knowledge concerning the negative effects of implants, the essay points to several problems exacerbated by breast implants: decreased detection of tumors, increased number of unnecessary biopsies, and possible promotion of scleroderma-like difficulty in swallowing in breast-fed infants. Ferguson's essay also highlights the degree to which lack of informed consent is embedded in accepted medical standards of care.

Coeditor Kasper was a founding member of the National Women's Health Network in 1976 and is currently at the Center for Research on Women and Gender at the University of Illinois at Chicago. In the sub-

section on women, their bodies, and the illness experience, Kasper uses her own research on a small group of urban poor women to illustrate the complex and destructive intertwining of our country's lack of universal health care, our current health insurance and health care systems, poverty, socioeconomic class, racial/ethnic discrimination, and the increasing inequality of income in the United States.

While every essay in this collection provides well-documented analyses of portions of the larger picture, as a feminist scientist and breast cancer activist, I particularly value "The Environmental Link to Breast Cancer" by scientist and poet Sandra Steingraber and "Sister Support: Women Create a Breast Cancer Movement" by former lawyer Barbara Brenner of San Francisco's Breast Cancer Action, a grassroots not-for-profit that "speaks truth to power." Indeed, prominent among the dozen specific and fundamental changes Kasper and Ferguson list in their agenda for lowering the incidence and mortality rates of breast cancer are "making prevention rather than cure the goal of both research and clinical practice, . . . eliminating corporate interests in making profits from the disease, [and] breaking the silence about environmental connections to breast cancer" (356).

Zillah Eisenstein's *Manmade Breast Cancers* journeys through the same challenging territory as the former collection. Eisenstein decided, however, to write a personalized political memoir; she, her mother, two of her sisters, and her aunt have had breast cancer (or in her aunt's case, ovarian cancer), and only she and her mother have survived. "I share my family story not as melodrama with victims and sadness but as a rich location for a personally passionate politics that includes the wider environments of breast tissue" (ix). Eisenstein's life as a feminist theorist and her Marxist skepticism of individualistic solutions to systemic problems "rooted in consumerist profit-making institutions" (2) change what would be a moving illness narrative into a more powerful treatise that attempts "to turn the political agenda across the globe toward the health of the breast. I use my body's story to restart, again, the ongoing project toward antiracist feminisms that can challenge exploitation and degradation across the globe" (ix).

Eisenstein intertwines her intellectual history with her effort to theorize from the body, her body—or her "bodies" (surviving a car accident and the ensuing endocrine dysfunction, a precarious pregnancy, childbirth, fighting breast cancer and chemotherapy, and aging). Looking back at herself in her twenties, she remembers the attraction of feminist and Marxist theories that reject the body as a biological determinant or constraint, and she recalls her ability to deny the materiality of our physical beings.

Age and self-reflection allow her “to see the power of ideas and how they impact on the way we see, or do not see” (47). She applies feminist theorizing to understand breast cancer “and with it, our world” (ix). And she uses the uncomfortable language of cancer to advance feminist theorizing. “Theory is a way of seeing connectedness—of the breasts to the rest of the body; of the body to the rest of its environments; of the historical process over time, which triggers cancer mutations, to the fluidity of borders between the breast and all else. Theory allows me to see beyond singularity and inevitability” (ix).

Her acknowledgments reflect the book’s scope, from feminist scientists who explained the science, to the organic farm where she shops, to the Ithaca Breast Cancer Alliance that provides support and advocacy for those affected by the disease, to projects about breast cancer in Cuba and China. Writing with searing passion and a sustained radical analysis, she chooses her extensive examples and scientific evidence carefully, always contextualizing ideas within a broader theoretical picture. In one important case among many, she summarizes the predominant “alternative” views about the role of diet in “fool[ing] the process of mutation,” at the same time pointing out that such concerns are “big business” (93), whether for the organic food industry or for the supplements industry, which ignores the prevalence of pesticides. Most of her sources in science and science studies are sound. However, on occasion she misses the mark on scientific evidence, such as citing (on page 94) Robert Arnot’s *The Breast Cancer Prevention Diet* (New York: Little, Brown, 1998), a best-seller by a media doctor. Based on exaggerated and sometimes inaccurate reporting of scientific studies, Arnot’s work was soundly criticized by the National Breast Cancer Coalition for promising “prevention” on the basis of little evidence.

Both her style and her choice of a personalized political memoir allow her to be very clear about her assumptions, values, and conclusions. Citing the push to use tamoxifen on healthy women to “prevent” breast cancer, Eisenstein condemns those who control treatment and “prevention.” “Breast cancer is big business. Many of the same corporations that contaminate our bodily environments sell the drugs that are supposed to prevent malignancy. Zeneca manufactures pesticides at the one end and markets tamoxifen at the other” (124–25).

Her scathing remarks might offend some readers, but her assessments are difficult to deny: “The postindustrial-medical complex has woven a complex directorate of implicit deceit and misrepresentation of the estrogen-breast cancer-tamoxifen connection. Environmental biogenetics has been crowded out by a medical politics deeply embedded in pharmaceu-

tical and chemical corporations hell-bent on selling an estrogen blocker that itself may be carcinogenic" (124).

Eisenstein and the authors in *Breast Cancer: Society Shapes an Epidemic* cover much the same ground, but with different vehicles and tones. Both books would be valuable additions to science and society or feminist science studies courses, with Kasper and Ferguson's being particularly useful in health policy courses. I would recommend *Manmade Breast Cancers* and its more global scope for feminist theory courses to explore how theorizing shapes political activism, keeping in mind that it may be less effective as outreach to nonfeminists and less politicized feminists. I hope that these books will stimulate additional feminist writings that address more of the many specific issues that require analysis, such as whether screening mammography actually saves lives and how we could change that practice. Meanwhile, this range of authors, the scope of their analyses, and their concluding agendas for change provide a much-needed framework for contemporary feminist analysis of the health of the planet. ■

Athena Unbound: The Advancement of Women in Science and Technology. By Henry Etzkowitz, Carol Kemelgor, and Brian Uzzi. Cambridge: Cambridge University Press, 2000.

Women, Science, and Society: The Crucial Union. By Sue V. Rosser. New York: Teacher's College Press, 2000.

Women Becoming Mathematicians: Creating a Professional Identity in Post-World War II America. By Margaret A. M. Murray. Cambridge, Mass.: MIT Press, 2000.

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Pamela S. Lottero-Perdue, *University of Delaware*

Becoming a scientist or mathematician can be thought of as a process of identity construction. Identity refers to the practices in which one participates and how others interpret and respond to these forms of

participation.¹ An individual negotiates her identity by, first, making a claim on her identity, and second, judging the viability of that identity against the reaction of others.² Taking on the identity of a scientist or mathematician is thus both cognitive and social because it requires the acquisition of new competencies that are then performed and evaluated by self and others within the scientific or mathematical community. Newcomers are enculturated into scientific or mathematical communities by participating in and acquiring competence in their practices. There can, however, be barriers that prevent learning and full participation and thus also limit who can and cannot become accepted participants in the scientific or mathematical community. In these communities, barriers have been both gendered and racialized, privileging white males who have access to elite academic institutions.

All three books reviewed here provide insight into the gendered nature of becoming a scientist or mathematician. The focus of Margaret Murray's *Women Becoming Mathematicians*, and *Athena Unbound*, by Henry Etzkowitz, Carol Kemelgor, and Brian Uzzi, is the process of becoming a scientist or mathematician. Both books examine the barriers women face in fields that appear to require an early and uninterrupted commitment to mathematics or science. It is presumed that important work in these fields is done before the age of forty and that a scientist or mathematician is willing and able to forgo all other activities during this time. In other words, this career trajectory was constructed by and for privileged males in conventional patriarchal families who have the luxury of having a single focus on their research.

In *Women Becoming Mathematicians*, Murray, a mathematician, uses interviews with thirty-six women who obtained their Ph.D.s in mathematics in the 1940s and 1950s to describe the lifelong process of negotiating a mathematician's identity. The circumstances of these women's individual careers are examined in light of larger social forces. For example, prior to World War II, few professional women attempted to be mothers

¹ See Jean Lave, *Cognition in Practice* (New York: Cambridge University Press, 1998), and Etienne Wenger, *Communities of Practice: Learning, Meaning, and Identity* (New York: Cambridge University Press, 1998), for theoretical background on learning as identity formation.

² See Erving Goffman, *Presentation of Self in Everyday Life*, monograph no. 2 (Edinburgh: University of Edinburgh, Social Sciences Research Center, 1958). Goffman describes such everyday occurrences as performances, during which one tries on desired identities. See Nancy Brickhouse, Patricia Lowery, and Katherine Schultz, "What Kind of a Girl Does Science? The Construction of School Science Identities," *Journal of Research in Science Teaching* 37, no. 5 (2000): 441–58, for how these identities are tested in science classrooms.

and mathematicians. Most never married. However, post-World War II U.S. culture valorized traditional families, and many women of this generation faced the challenge of simultaneously constructing identities of mathematicians, mothers, and wives, with very few role models or mentors to help. Since many of these women married mathematicians or other academics, antinepotism laws at colleges and universities further constrained their attempts to engage fully in mathematical communities.

Murray clearly recognizes the significance of understanding mathematics as inherently social. While mathematics can be done in a solitary way—one of the interviewees describes doing research mathematics on her sofa while tending to young children—most recognize that frequent, substantive contact with other mathematicians is an essential component of engagement in mathematics. For women during their child-bearing years, maintaining this kind of engagement with the mathematics community was among their greatest challenges. Advisors, mentors, and occasionally mathematician husbands could be helpful in getting or maintaining an identity within the mathematics community.

The strength of Murray's book is that it is meticulously documented, engaging, and optimistic. Murray's use of oral histories is qualitative research at its best because she provides an understanding not only of what is typical but also of the complexity of individual women who negotiate barriers in very different ways in order to gain access to the mathematics community. In doing so, she offers hope to women readers pursuing nontraditional fields and to those who educate them that such barriers are surmountable. Some of the women in the study are clearly more successful than others, and some more readily identify themselves as mathematicians; however, Murray emphasizes that for these women there was not a single best route to *becoming* a mathematician and there were a variety of ways to *be* a mathematician. Murray is careful in recognizing the varieties of ways of becoming/being a mathematician without valorizing any one aspect of participation. Her descriptions clearly place all these women in a place of honor.

Athena Unbound is based on a large body of data collected in the late 1990s, including interviews with science and engineering undergraduate students and faculty members, a survey of graduate students, and interviews with young children. Although there is some discussion of early educational experiences, the book focuses primarily on identity construction in science and engineering in the postsecondary years.

Although *Athena Unbound* is based on the experiences of a much younger group of women in the sciences and engineering, many of the barriers these women faced are strikingly similar to what is described in

Women Becoming Mathematicians. Three common barriers are the following: the career trajectories of women mathematicians are expected to match those of their male colleagues, without consideration for interruptions due to childbearing and rearing; the fact that universities rarely make much effort to accommodate academic couples, despite the abolition of antinepotism laws; and overt discrimination remains problematic for many women scientists and engineers during their education and careers. Also addressed in *Athena Unbound* and *Women Becoming Mathematicians* is the significance of choosing an advisor whose work is sufficiently current that one can generate one's own research program from a foundation established in graduate school. These books also emphasize that equally important to the development of one's research program is the establishment of social contacts within and outside the department.

There are some differences in mathematical and scientific work that reading these books in tandem highlight. Much mathematics research even today requires very little capital investment, especially when compared to science. Within some areas of mathematics and unlike scientific research, it is still possible to do mathematics research at the kitchen table. This made it possible for many of the women in Murray's book to continue to participate in research even when they were unemployed or underemployed. Theoretically, this should make reentry into the field much easier. Women scientists would not be able to engage themselves in a similar fashion during such a hiatus. The technology that is routinely required for scientific inquiry makes a kitchen table a rather unsuitable locale for extended research in the areas of physics, chemistry, or biology. Despite this added challenge for women scientists, it is still the case that the percentage of women scientists is greater than the percentage of women mathematicians. This suggests that a myriad of factors influences the participation of women in mathematics.

One commonality between Murray's work and the work of Etzkowitz et al. is that both books focus on elitist universities as the sites of education and, as a result, focus on the kind of science or math that is done in these institutions. We must also recognize that math and science are done in many other places that would not be considered "elitist." However, if we can forgive the narrow focus, these books provide important insights into the process of women becoming scientists—from both current and historical perspectives.

Whereas *Women Becoming Mathematicians* and *Athena Unbound* substantively address identity formation that includes or excludes women from scientific and mathematical communities, neither thoroughly addresses the

issue of how the fields might benefit cognitively from the presence of women who can bring feminist perspectives to the fields. Sue Rosser's book, *Women, Science, and Society*, examines the research currently done in the biological sciences, especially genetics, health, and medicine, from a postcolonial feminist perspective—a perspective she elucidates in her book. In many respects, *Women, Science, and Society* is optimistic. Women have entered the life sciences and medicine in unprecedented numbers. In addition, women are actually very near parity with men with regard to Ph.D.s awarded in the biological sciences. Rosser recognizes the opportunities this creates for women in the biological sciences to move well beyond tokenism to make a serious impact on the field.

Although Rosser identifies the ways in which women can have a significant presence in the biological and health-related fields, she also examines the ways in which research trends in these fields have ignored or marginalized women and, perhaps most critically examined by Rosser, created hardship for women in third-world nations, previously colonized countries, and other places that fall under the influence of powerful Northern/Western nations. She discusses how feminist approaches might change research when the experiences and needs of women within and outside of Northern/Western science are taken as a central concern. Although Rosser is quite critical of much of the current research trends in biology, it is evident that she holds scientific work in high esteem. She believes that lives can and should be improved as a result of women's entry and participation into the life sciences.

Women Becoming Mathematicians is likely to be of high interest to mathematicians and mathematics educators. *Women, Science, and Society* is likely to be of most interest to feminist scientists and science educators. *Athena Unbound* could be appropriate for both advanced undergraduate students in the sciences who are setting out a strategy for their own careers and for department chairs and others seeking to recruit or retain more women. It is a frustrating book, however, because the authors have not been very careful in documenting the evidence on which their claims are based. Huge amounts of data were collected for this book, but no description of how these qualitative and quantitative data were analyzed to form the substance of this book is provided. Although we actually agree with most of the claims made here, skeptics could easily dismiss them. Yet all three of these books address important aspects of becoming and being mathematicians and scientists. 1

Gender and Boyle's Law of Gases. By Elizabeth Potter. Bloomington: Indiana University Press, 2001.

Feminism in Twentieth-Century Science, Technology, and Medicine. Edited by Angela N. H. Creager, Elizabeth Lunbeck, and Londa Schiebinger. Chicago: University of Chicago Press, 2001.

Revisioning Women, Health, and Healing: Feminist, Cultural, and Technoscience Perspectives. Edited by Adele E. Clarke and Virginia L. Olesen. New York: Routledge, 1999.

Wild Science: Reading Feminism, Medicine and the Media. Edited by Janine Marchessault and Kim Sawchuk. London: Routledge, 2000.

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At the beginning of the twenty-first century, as this wave of feminism and women's studies marks its third decade, the cross-fertilization of the interaction among science, technology, medicine, feminism, and women's studies has begun to blossom and bear fruit. Unlike the humanities, where much of the impetus for women's studies originated and where feminist scholarship has become mainstream in most disciplines, and even the social sciences, where gender analyses and questioning of positivist approaches have become routine, science, technology, and medicine have come to accept feminist perspectives and gender analyses more slowly. In the past, many characterized the interaction as something like the mixing of oil and water. In 1988, I expressed my own frustrations and feelings of isolation as one of the few scientists who attended women's studies conferences and one of the few feminists and women's studies scholars at science conferences: "For years I have always felt an outsider at national professional meetings in either science or Women's Studies. At the science meetings I was usually the only, or one of a very small group of feminists interested in feminist questions and/or critiques of science. At women's studies meetings I was usually one of a very small group of scientists interested in scientific questions within feminism."¹ Those of us who had one foot in science and the other in women's studies

¹ Sue V. Roeser, "The Impact of Feminism on the AAAS meetings: From Nonexistent to Negligible," in *Feminism within the Science and Healthcare Professions: Overcoming Resistance*, ed. Sue V. Roeser (Elmsford, N.Y.: Pergamon, 1988), 105.

worked hard to build the two-way streets between science and feminism that were first articulated by Anne Fausto-Sterling.²

In the late 1980s, when I edited a special issue of a journal on the topic of feminism and science to honor Ruth Bleier, finding sufficient papers proved an arduous task.³ Almost all the papers published came from biologists and philosophers and focused on critiques of science, the current or historical status of women within the profession, or curricular and pedagogical resources and approaches. Editing a volume on the same topic in 2000, not only did I receive an overwhelming response to the call for articles, but the authors came from physics, geology, engineering, chemistry, and computer sciences, as well as the humanities, social sciences, and women's studies.⁴ In addition to the continuing focus on critiques, status of women within the profession, and curriculum, new categories—of gender differences and diversity among women, revealing male subtexts and building alternative models, theory into practice, and feminist science studies—had emerged. The tremendous increase during the 1990s in sheer volume of response, diversity of disciplines, and expansion of topics signals the intellectual excitement and interdisciplinary potential emerging from interactions among feminism, science, technology, and medicine.

The four volumes reviewed here take the interaction and discussion to the next level. Each in its own way evolves from and responds to questions raised by earlier work and by scholars in feminist technoscience studies. The book *Gender and Boyle's Law of Gases* developed from author Elizabeth Potter's response to a challenge raised at a talk on gender and science given by Evelyn Fox Keller in the early 1980s at the Boston University Philosophy of Science Colloquium. An audience member's statement, "Yeah, but you'll never show that gender affects something like Boyle's Law!" (ix), led Potter to respond with a demonstration that not only gender but also other social conditions, such as Boyle's conservative political and religious beliefs, influenced Boyle's choice of a theoretical model to explain his Law of Gases.

In the first part of the book, Potter documents that Boyle had a strong interest in gender, femininity, and women, despite his life as a bachelor. Although Boyle wrote extensively about gender and the appropriate

² Anne Fausto-Sterling, "Building Two-Way Streets: The Case of Feminism and Science," *NWSA Journal* 4, no. 3 (1992): 336–49.

³ Sue V. Rosser, ed., *Women's Studies International Forum* 12, no. 3 (1989).

⁴ Sue V. Rosser, "Twenty-Five Years of NWSA: Have We Built the Two-Way Streets between Women's Studies and Women in Science and Technology?" *NWSA Journal* 14, no. 1 (2002): 103–23.

"modesty of body," as well as about the chastity and silence (83) he expected of women, he separated his writing about gender from his writing about science. Potter re-creates the historical period in which Boyle conceived his law and undertook his experiments in the second part of the book; during the civil war that raged in the 1640s, radicals threatened not only the absolute monarchy and decision-making authority and wealth of the king, church, and upper classes but also of men. The petitions presented by women claimed political equality with men (75) and led to the establishment of civil marriage. Potter claims that because, as an upper-class male, Boyle was personally threatened by, and opposed to, the radicals, he rejected the animism that he saw as linked with radicalism. He chose the mechanistic model both because it comported well with the data (108) and because it supported proper religion, monarchy, and the status quo with regard to class and gender. The same data could have been used to support an alternative law offered by Boyle's contemporary Franciscus Linus. Boyle did not clearly refute Linus's hypothesis on methodological or experimental grounds (154).

Just as feminists have demonstrated that androcentric bias infiltrates research because these values are shared by many in the scientific community, Potter suggests that Boyle's biases influenced his work, which was then accepted by the scientific community that also shared his values. His work also represented "good science," although the experimental evidence of the time did not permit distinction between Boyle's and Linus's hypotheses.

In short, Boyle's conservative social, political, and gender values influenced his choice of the mechanistic model and his Law of Gases, thus demonstrating that Boyle's Law was not free of contextual values but that it was still good science. Through careful analyses of historical documents, Potter met the challenge raised at that conference in the early 1980s to demonstrate that gender affects even something like Boyle's Law.

The demonstration that contextual values, including gender, bias not only the scientific research of individuals but also what is accepted as valid science by the entire scientific community represents one of the major contributions that feminism has made to science. In *Feminism in Twentieth-Century Science, Technology, and Medicine*, contributing authors respond to the question, What difference has feminism made to the fields of science, technology, and medicine? This volume originated from a conference held by the Women's Caucus of the History of Science Society at Princeton in 1998. It builds on coeditor Londa Schiebinger's 1999 book *Has Feminism*

*Changed Science?*⁵ In that volume, Schiebinger examined how the presence of women in traditionally male disciplines has altered scientific thinking and awareness, concluding that feminist perspectives have had little effect on mathematics and the physical sciences but more impact on biology, including medicine, archaeology, reproductive and evolutionary biology, and primatology.

As the title *Feminism in Twentieth-Century Science, Technology, and Medicine* suggests, the coeditors of this volume expand the question beyond science to technology and medicine of the twentieth century. Chapter authors include premier feminist scholars in the subdisciplines: Alison Wylie on archaeology, Linda Fedigan on primatology, Evelyn Fox Keller on feminist critiques of science, Caroline Pursell on the history of technology, Emily Martin on medical language, Ruth Schwartz Cowan on medical technologies, Nellie Oudshoorn on essentialist views of bodies, and Evelyn Hammonds on HIV/AIDS in women. The coeditors insisted that authors emphasize feminism, not simply women and gender, because "participation of women does not equal transformation" (3).

The answer of each author to the question varied both because of the degree of transformation of the specific subdiscipline analyzed and because of the particular angle or perspective used. Cowan, using the brilliant insight of the "consumption junction" from her earlier work on women and technology, finds that the impact of women on technology remains in their role as consumers, rather than designers: "And shortly after *that* I realized that my new research—into the history of prenatal diagnosis—could benefit not only from systems thinking but from feminist systems thinking: the consumers of this new technology were, after all, *all* female" (189).⁶

In contrast, Fedigan details the eight tools of gender analysis developed by women in primatology: scientific priorities, representative sampling, dangers of extrapolating research models from one group to another, institutional arrangements, gender dynamics in the cultures of science, language use, the remaking of theoretical understandings, and challenges to what "counts" as science. She then ends by examining why so many

⁵ Londa Schiebinger, *Has Feminism Changed Science?* (Cambridge, Mass.: Harvard University Press, 1999).

⁶ Ruth Schwartz Cowan, "The Consumption Junction: A Proposal for Research Strategies in the Sociology of Technology," in *The Social Construction of Technological Systems: New Directions in the Sociology and History of Technology*, ed. Wiebe E. Bijker, Thomas P. Hughes, and Trevor J. Pinch (Cambridge, Mass.: MIT Press, 1987), 261–80.

women primatologists refuse to identify as feminists, despite the predominance of women in the discipline.

Although the degree and the specifics of the impact of feminism on science, medicine, and technology vary from one subdiscipline to another, the coeditors of *Feminism in Twentieth-Century Science, Technology, and Medicine* state the uniting bottom line in the introduction: "Feminism connects gender to other systems that structure our lives and individual identities" (viii). *Revisioning Women, Health, and Healing: Feminist, Cultural, and Technoscience Perspectives* pushes the same question of how feminism has influenced science in a slightly different direction and into the applied area of health. This volume originated yet again from a conference, held in 1998 to celebrate the "twenty-something" years in women's health of the Women, Health, and Healing Program at the University of California, San Francisco, through a "radical revisioning of the entire domain of women, health, and healing" (ix). It focuses on how women's health has been changed and should be changed (i.e., what should be the new agenda for women's health?) in light of recent insights from feminist theories and increasing technoscience interventions in health care.

After the editors' introductory overview chapter, Donna Haraway contributes the first chapter. She was chosen to deliver the keynote address because of her stature as a feminist scholar, and her image of the cyborg embodies the extent to which technoscience interventions have become part of us and of women's health. She uses the image of the "virtual speculum" from her own 1997 book *Modest_Witness@Second_Millennium: FemaleMan_Meets_Oncomouse: Feminism and Technoscience* (New York: Routledge, 1997) to "open up observation into the orifices of the technoscientific body politic to address these kinds of questions about knowledge projects" (67). Through the opening, she sees the diffractions of reproductive health in modern global society, where infertility has become the health problem for women of certain races and classes in developed countries, while overpopulation is defined as the problem for women of other races and lower socioeconomic status in developing countries. Simultaneous with this overpopulation, the culture of breastfeeding has been lost in the favelas in the Nordeste of Brazil, where the father's provision of milk symbolizes paternity. To breast-feed her baby signifies that the woman has been abandoned by the baby's father. The adoption of some "modern" health practices such as bottle-feeding simultaneously with the nonadoption of others such as contraception demonstrates the role of culture in mediating these diffracted reproductive health practices.

Emily Martin concludes the section of the volume devoted to "theo-

retical speculations" with her insightful analysis of language that reveals that we are in the process of a sea change regarding how the body is conceptualized. The older, steadier mechanical industrial model has begun to give way to a fluid, complex systems model, exemplified in the new theories of the immune system.

Four other sections of the book include chapters by well-known interdisciplinary feminists with expertise in each wide-ranging category relevant to women's health: "Destabilizing Methods" includes very useful chapters, particularly for health care researchers doing qualitative, ethnographic research, such as Rayna Rapp on multisited ethnographies of reproductive technologies, Patti Lather as a chronicler for women with HIV/AIDS, Denise Segura and Adela de la Torre on variations in Latina health as a function of acculturation, and Marjorie DeVault on women's stories as professionals in public health nutrition. "Searching the Self" contains personal reflections by Sharon Traweck on her gynecological experiences in Japan and the United States, Ruth Behar on the impact that spending a year immobilized in a body cast at age nine had on her then and now, and Françoise Vergès on how (post)colonial psychiatry inappropriately translates Eurocentric, industrialized expectations onto Creole society on Reunion Island.

"The New World Reproductive Order" uses literature (Anne Balsamo employs Margaret Atwood's *The Handmaid's Tale*) and software and advertisements (Valerie Hartouni analyzes *The Nine Month Miracle* and an Evian ad) to understand the extent to which new technologies permeate reproduction.⁷ Patricia Hill Collins and Beth Richie in their respective chapters explore the overlay of race onto policies surrounding family planning and what and who defines "good mothering." "Revised Agendas" puts forward new agendas for women's health. Sheryl Ruzek reminds us that we "can't have it all" (305) in terms of universal access to health care, cost containment, and individual choice. Since the decision of what parts we must have comes down to values, feminists must weigh in. Jennifer Terry describes the issues in health care for lesbians, the overlooked, understudied, and undertreated population; Nancy Woods suggests that the disconnection between the way women view menopause as "a change" while physicians view menopause as "a disease" may also result from inadequate study of the overlooked population of aging women.

In conclusion, Clarke and Olesen underline that a significant way feminism has changed women's health is through diffraction. Diffractions

⁷ Margaret Atwood, *The Handmaid's Tale* (Boston: Houghton Mifflin, 1986); *The Nine Month Miracle*, CD-ROM produced by A.D.A.M. Software, Inc., 1994.

through differing feminist theories permit the simultaneous holding of the binary tensions—cross-cultural and local studies, difference and equalities, medical professionals and health care activists, traditional and alternative medicine, communities of risk and cultural competence—that must be balanced in creating a new agenda for women's health.

While continuing to explore the question of how feminism has changed health, *Wild Science: Reading Feminism, Medicine, and the Media* begins to travel the two-way street in the other direction. Coeditors Janine Marchessault and Kim Sawchuk recognize that “science is made of practices that belong to the culture” (2), including the practice of gender, as demonstrated by feminist science studies. They sought a group of essays unified in their focus on the visual culture of medicine.

Using the technologies, particularly the visual or imaging technologies, of science or medicine, each author elucidates key aspects of feminism, gender, and cultural constructions. Three chapters explore corporeal maps, the use of visual technologies to map the human body, as represented in popular culture: the movie *Fantastic Voyage* as a form of biotourism; the “Visible Human Project” that reveals the body through digital simulation; and brain scans used to elucidate false memories from “truth.” The media coverage of people's reaction to each not only entrenches it in popular culture but also affects the evolution and use of the visual technology itself.

A section of the book devoted to genetic codifications continues to examine the reciprocity between science and media. Marchessault looks at the “The Secret of Life” series by Canadian broadcaster, geneticist, and environmental activist David Suzuki to see whether, in this age of genetics as the life code, culture is biology. José Van Dijck analyzes metaphors in genome research, concluding that since researchers transpose literature on to biology, it is not possible to critique science without critiquing culture. In a compelling chapter, Bonnie Spanier uses the TV show *Ellen* and the question “What made Ellen gay?” as a springboard to explore the current obsession with finding a “gene” for gayness, despite inadequate experimental evidence for such genes and the lengthy historical tradition of failed attempts to find “biological causes” for homosexuality.

At first glance, the five chapters devoted to “clinical practice” appear as an eclectic group of topics ranging from plastic surgery and HIV through breast cancer activism to reproductive issues raised by RU-486 and the ability of postmenopausal women to give birth using assisted reproduction. The theme that unites these chapters and the entire volume is these topics' depiction by the media, which then modifies or affects the science, or in this case, clinical practice itself. The book ends with a won-

derful letter written to a graduate student by revered Canadian physicist Ursula Franklin. She spells out the importance for feminists to become scientists who study ignored topics, reveal biases, and expand the limits of reductionistic approaches to the levels that make the work significant for human beings.

The final section of the book uses feminism to juxtapose humanities and science in 1990s academia. In "Teaching in the Belly of the Beast," Anne Balsamo provides a primer for teaching feminist technoscience studies to engineering students, which she taught in a course in a school of humanities within a technological institution (Georgia Institute of Technology). She attempts to teach students to remain critical of the practices and institutions of science and technology, while supporting their (especially the women's) choice of a career in science or engineering.

Jennifer Slack and M. Mehdi Zimanti demonstrate that the hoax perpetrated on the journal *Social Text* by physicist Robert Sokal represented a political attack that extended beyond science studies to feminism. They conclude that the perpetrators of the Sokal hoax operated from a deep fear that science would not remain safe from the corruption of the humanities.

Although I do not fear that the sciences will be corrupted by the humanities, these four volumes suggest that if the source of such fear is increased interaction among science, technology, medicine, feminism, the humanities, and women's studies, then the fear is justified. In contrast to Sokal, I celebrate this interaction and blurring between the sciences and humanities and see feminist technoscience studies as the direction for women's studies in the twenty-first century. Just as women's studies will be broadened and enhanced by such increased interdisciplinary interaction, so will science, technology, and medicine be enriched by feminism and collaboration with colleagues in women's studies. 1

Biology at Work: Rethinking Sexual Equality. By Kingsley R. Browne. New Brunswick, N.J.: Rutgers University Press, 2002.

Reinventing the Sexes: The Biomedical Construction of Femininity and Masculinity. By Marianne van den Wijngaard. Bloomington and Indianapolis: Indiana University Press, 1997.

Lisa H. Weasel, *Portland State University*

There is nothing new about the sociobiological claim that inequality between men and women results from evolutionary forces and sexual selection. Nor are there any new data or research on the subject in *Biology at Work* by Kingsley Browne. As the author himself states with regard to the lack of novel findings or data, "especially is this so in a work such as this, which does not convey any original research findings but rather seeks to synthesize findings of others from disparate disciplines" (acknowledgments).

What is new is Browne's determined yet flaccid attempt to draw sociobiology into the workplace, to account for such disparities as pay inequity and sexual harassment, and to justify and even extol situations such as the underrepresentation of women in the physical sciences. Browne's arguments are based on his conjecture that women and men differ psychologically, that this sexual psychological dimorphism is solely biologically ingrained (there is no discussion of social forces affecting human psychology, with the exception of curt and occasional dismissals), the result of cons of evolutionary selection working primarily through hormones, and that the resulting disparities and differences we see in the workplace are only an expected consequence of our evolutionary biology. Sexual harassment at work? It is only a natural outcome of men's biologically based urge to seek out nubile, single females ("one would expect the ideal victim to be single, divorced, widowed, or, if married, married to a man whom the potential harasser believes not to be a threat" [203]). Browne titles one section "Sexual Harassment as a Mating Strategy" (202). And, in a tired but familiar vein, Browne suggests that women's own actions cause them to fall prey to men's only natural inclinations: "for example, heavy use of cosmetics is often taken, by both men and women, as a sign

of lower levels of morality. Heavy makeup may therefore be interpreted as signaling receptivity to sexual advances, a desirable trait in short term mates" (194).

He views women's disproportionately low representation in areas such as blue-collar occupations and the mathematical and physical sciences as biologically based, and, as a result, he is harshly critical of programs that aim to encourage girls and women to participate in math and science, suggesting that beyond being illegal they pose the "fundamental question of whether female scientists are 'worth more' to society than male scientists, to justify the cost of producing them" (149). The math connection? Harder to explain, says Browne: "mathematical ability, in contrast, may not have been selected for directly, but it may be a byproduct of enhanced spatial ability" (117).

It is just these kinds of logically satisfying answers that make Browne's book less of a novel threat to feminist positions on sex discrimination in the workplace and more of a straw man, set up by the author himself, awaiting self-immolation. Much of the logic in this book is typified by the following example, in which Browne attempts to support his hypothesis that gendered behavior is hormonally based by drawing on androgen insensitivity syndrome (AIS). "AIS individuals have the XY chromosomal complement of a male, and their testes develop and produce testosterone, but their tissues lack androgen receptors. AIS patients tend to exhibit stereotypically female preferences (such as a desire to be a wife with no outside job) and tend to be interested in infants and dolls." So far, so good for Browne, until in the next sentence he concedes that "interpretation of these findings is complicated, of course, by the fact that the subjects are also reared as girls" (111). Must be the hormones. Watch out for them at work.

Published in 1997, at the height of feminist theory's infatuation with postmodern deconstruction of that thing called "the body," Marianne van den Wijngaard's *Reinventing the Sexes* does not seem particularly novel either, but books like Browne's prove that this and other works like it are still much-needed resources for critically examining the concepts of sex and gender and their scientific construction. In contrast to Browne's sociobiological missive, van den Wijngaard's introduction claims that "this story is not about determining who is right or wrong" (5), although it engages a similar subject, the organization theory that contends that gendered brains are organized in response to hormone exposure in utero. Instead, this book asks and answers fundamental questions about the origins of these ideas, such as, "How did scientists and physicians construct

dualistic images of femininity and masculinity by producing knowledge based on the organization theory? How did feminism help stabilize or change these images?" (20).

The opening chapter consists of a comprehensive survey of feminism's critical relationship with the scientific study of sex and gender differences, with appropriate citations of both feminist scientists and philosophers. In the next two chapters, van den Wijngaard launches into a detailed historical analysis of how the organization theory arose, related to the need in both biology and psychology to find an explanation for increasingly entrenched, dualistic gender roles: "the dualistic beliefs about masculinity and femininity that prevailed in western society during the 1960's, which were manifested in the dualistic role ascribed to gonadal hormones, determined the course that research was allowed to take" (45). When feminists and female scientists entered the field (van den Wijngaard takes the view that "a distinction between feminist scientists and women scientists is hard to make" [75]) things changed slightly. Whereas in an earlier era, "researchers viewed female animals as instruments for investigating male sexual behavior" (52), now female sexuality was fair game for study as well. Van den Wijngaard describes this change with an example in which a female researcher "tied down male rats and gave female rats the opportunity to react to the tied males. Her video demonstrated behavior that had previously been invisible . . . to their amazement, the viewers witnessed the female rats display a comprehensive repertoire of sexual behavior" (55). Alas, van den Wijngaard contends, although feminism helped to change the questions that were asked, most of the feminist-inspired research did little to change the dualistic ideology, if anything reinforcing it: feminist ideas "could become integrated as long as they contributed to the idea of a dichotomy between the sexes and subscribed to the prenatal hormone paradigm" (79).

After closely tracking the development and acceptance of the organization theory, van den Wijngaard turns to its practical application: the management of intersex (she uses the term *hermaphrodite*) individuals. Here the author treads on tricky ethical ground, as most feminists who have invoked intersexuality in support of sex/gender deconstruction completely fail to address the ethics of surgical intervention or the lived reality of intersex individuals. To her credit, van den Wijngaard acknowledges that many intersex people's lives have been harmed by surgical intervention and refers to support groups that have been established in this regard. Yet one wishes that her rather timid recommendation—"In evaluating such treatments, I would question if a cultural context that makes it hard to exist without clearly belonging to one sex or the other is more important

than taking the risk of multiple surgeries and thereby removing the possibility for sexual pleasure" (95)—would carry more weight.

Overall, the book offers a comprehensive evaluation of how dualistic ideology about sex, gender, and hormones became imbedded in science and in clinical practice, and the role of feminism in this process. Understanding this history is undoubtedly crucial background for any feminist who wishes to engage in the process of "reinventing the sexes." 1

Has Feminism Changed Science? By Londa Schiebinger. Cambridge, Mass.: Harvard University Press, 1999.

Feminist Science Studies: A New Generation. Edited by Maralee Mayberry, Banu Subramaniam, and Lisa H. Weasel. New York: Routledge, 2001.

Mary Wyer, North Carolina State University

Not so long ago, Helen Longino posed the question, "Can there be a feminist science?" and answered it with a "yes and no." Yes, it is theoretically possible to practice science as a feminist, but the current social and political contexts in which scientists work make it impossible to do so.¹ Both *Has Feminism Changed Science?* and *Feminist Science Studies* offer more optimistic stances, albeit with quite different understandings of the purpose of bringing feminist perspectives into scientific research.

Londa Schiebinger is a historian of science. She turns her skills as a careful, methodical scholar to describing three distinct threads of work in feminist perspectives on the sciences—one that focuses on underrepresentation, one that focuses on the masculine culture, and one that focuses on content. The narrative tone is one of distant authority and engaged scholarship, so, for instance, the first-person "I" is not present. The heart of the book is the last section, which provides a summation of the contributions that gender studies (aka feminism) has made to correcting and redirecting research in medicine, primatology, biology, and physics/math. She sets up this summation with two sections that take on issues surrounding the shibboleth of "women in science," reviewing the history

¹ Helen Longino, "Can There Be a Feminist Science?" in *Feminism and Science*, ed. Nancy Tuana (Bloomington: Indiana University Press, 1989), 46–57, esp. 56, first appeared in *Hypatia* 2, no. 3 (Fall 1987): 51–64.

and sociology of women's underrepresentation in science as well as how men's control of scientific institutions has shaped conventions in professional practices and priorities that continue to discourage and exclude women.

Schiebinger assembles an impressive amount of evidence to support the idea that yes, indeed, feminist perspectives (and women) have changed science, at least in terms of the increasing presence of women and some of the content of scientific research. She presents a more pessimistic picture of change in the aggressive and competitive culture of science, where the clash between how women are socialized and how scientists are socialized presents a seemingly intractable barrier to women's full participation. Still, in general *Has Feminism Changed Science?* is an optimistic overview and progress report on the effort to incorporate feminist social and political commitments into the practice of science.

Then again, Schiebinger is a historian; she measures change over the long term. She does not address how the changes took place in the past so much as offer persuasive evidence that the changes did take place. The purposes of feminist inquiry in the sciences, then, are to enrich and redirect science itself. In contrast, the editors of *Feminist Science Studies* define the purposes of feminist inquiry in the sciences as more comprehensive—to enrich and redirect science, to be sure, but also to enrich and redirect feminist inquiry. As Banu Subramaniam puts it in her essay “And the Mirror Cracked: Reflections of Natures and Cultures,” “if science had been constructed as ‘a world without women,’ feminism was constructed as a ‘world without science’” (58).

The edited collection is replete with first-person accounts of the challenges confronting those who attempt to cross or collapse the nature-culture divide. Since our educational institutions are organized around this divide, how precisely we go about doing this is a big question. Attempts to develop courses or research agendas that draw from the sciences as well as the humanities and social sciences risk the disdain, dismissal, and discouragement of feminist scientists' colleagues from either side of the divide. Yet the thirty-seven contributors, who come from a mind-boggling variety of disciplines and professional settings (many of them science-side), testify to a growing community of determined educational innovators.

The writings are divided into four sections that move the reader from stories of the alienated identities of those who work across disciplinary divides, through accounts of incorporating scientific and feminist perspectives as mutually informing, through “how to” descriptions of the

new courses and pedagogical commitments that result, to a concluding section that provides examples of the transformative political significance of feminist science studies. Though some of the essays are better developed, longer, and more detailed than others, in general the contributions are solid and useful, many excellent. Karen Barad's "Scientific Literacy → Agential Literacy = (Learning + Doing) Science Responsibly," for instance, critiques the twin notions that students' scientific literacy can be enhanced by making courses more "relevant" and by adding material that contextualizes scientific knowledge (both are sometimes used as reasons to include material about women in the science curriculum). She argues that this is a little like the spoonful of sugar that makes the medicine go down, that a more fundamental rethinking of how science is presented to students is in order. Though she focuses on physics in particular, her ideas are useful more generally: that is, that our courses should include an examination of disciplinary prerogatives to say what counts as "nature" and "culture," that what we call "science" and what we call "society" are mutually constitutive and we should teach this, and that "what it means to do responsible science . . . requires that we learn how our practices come to matter" (242).

Maralee Mayberry's essay, "Reproductive and Resistant Pedagogies: The Comparative Roles of Collaborative Learning and Feminist Pedagogy in Science Education," is a welcome antidote to well-meaning reformers in science education who focus on making changes in how material is taught but not what material is taught. Mayberry lays out with care the grounding assumptions of collaborative learning approaches and compares these to feminist pedagogical commitments. She makes the argument that faculty can develop ingenious and complex classroom techniques for addressing differences among students' learning styles, but if they are still teaching students that women's lives are of marginal concern then their "reforms" become part of the problem, not the solution.

Michael Witmore, Sandra Harding, and Bonnie Spanier also have contributed mature and thoughtful essays to the collection. Witmore ("When the Mirror Looks Back: Nature in the Scholarship of the Humanities") has written an innovative rumination on how his disciplinary training in the humanities structures and constrains the content, context, and legitimacy of including material about the natural world in his work. Harding ("After Absolute Neutrality: Expanding 'Science'") ponders an expansion of the definition of "real science" to accommodate an understanding of knowledge claims as culturally produced and embedded but also by some measure "true." Spanier ("Your Silence Will Not Protect You": Feminist

Science Studies, Breast Cancer, and Activism") contributes a lively call to action for feminist science studies by promoting a coalition of feminist activists and breast cancer activists.

It is striking that however differently *Has Feminism Changed Science?* and *Feminist Science Studies* approach the task of incorporating feminist perspectives into the sciences, and however differently they define the feminist perspectives at issue, both volumes conclude with sections focused on social change by, for, and about women. Unfortunately, there is too little work in either volume on differences among women, despite assertions of the importance of race, ethnicity, and class in both. This is a silence that reflects the weaknesses of early women's studies scholarship, and it reflects a shortcoming of the field of feminist science studies in general.² We know how to fix it. Since both *Has Feminism Changed Science?* and *Feminist Science Studies* are likely to prove generative, we perhaps can take comfort in knowing that a genuinely inclusive feminist science is possible, in time, after all. ■

On Her Own Terms: Annis Montague Alexander and the Rise of Science in the American West. By Barbara R. Stein. Berkeley: University of California Press, 2001.

Improbable Warriors: Women Scientists and the U.S. Navy in World War II. By Kathleen Broome Williams. Annapolis, Md.: Naval Institute Press, 2001.

Patricia Moore, Green Mountain College

Monographs on women, gender, and science in the United States are still rare relative to work on European women, gender, and science. Barbara Stein and Kathleen Broome Williams add to the "foundational" literature on exceptional American women that encourages schol-

² For exceptions, see selections in Sandra Harding's *The Racial Economy of Science* (Bloomington: Indiana University Press, 1993) and work by Evelyn Hammonds, "Missing Persons: Black Women and AIDS," in *Words of Fire: An Anthology of African-American Feminist Thought*, ed. Beverly Guy-Sheftall (New York: New Press, 1995), 434-39, and "New Technologies of Race," in *Processed Lives: Gender and Technology in Everyday Life*, ed. Jennifer Terry and Melodie Calvert (New York: Routledge, 1997), 108-21.

ars to ask more complex questions about gender.¹ Stein's *On Her Own Terms* examines the life and career of Annie Montague Alexander, an early twentieth-century patron and participant in the development of evolutionary biology. Williams's *Improbable Warriors* offers snapshots of four navy women involved in the burgeoning enterprise of applied science during World War II. She discusses the careers and technical accomplishments of Mary Sears in oceanography, Florence van Straten in meteorology, Grace Hopper in computer science, and Mina Rees in distributing military contracts for scientific research. Neither Stein nor Williams has previously written about women or gender. Stein works as a vertebrate zoologist and Williams as a military and diplomatic historian. Thus, while their respective expertise shines through each work, their gender analysis is problematic.

Williams argues that each woman she examines "influenced the navy's ability to wage a modern war dependent on science" (x). In addition, she describes the technical aspects of their work in order to examine "how women functioned in jobs for which many believed they were mentally and temperamentally unsuited" (xi). Her dual agenda creates an uneven book that alternates between narrative description and technical detail. This strategy obscures larger analytical themes such as gender. Nevertheless, her writing sparkles when she writes about technology. Grace Hopper's passion for numbers and order comes alive as Williams describes the elemental steps involved in early computer programming and the difference between the first mechanical computer (Mark I) and the vacuum tube technology of ENIAC, the forerunner of our electronic computers.

Williams does an admirable job striving to integrate gender into her work, but in the end she employs the descriptive "yes, women participated" technique characteristic of literature in emerging fields.² Her first chapter summarizes women's military work during World War II. She covers familiar territory, pointing out the high numbers of women workers, their enthusiasm, the new technical and scientific skills they learned, and that the war did not have a lasting impact on the professions in which they participated. She concludes that Sears, van Straten, Hopper, and Rees were different from the bulk of military women because all had Ph.D.s in scientific fields.

¹ Sally Gregory Kohlstedt and Helen Longino, "The Women, Gender, and Science Question: What Do Research on Women in Science and Research on Gender and Science Have to Do with Each Other?" *Osiris* 12 (1997) 3-15.

² Joan Wallach Scott, *Gender and the Politics of History* (New York: Columbia University Press, 1988), 32.

In addition, Williams equates gender with feminism. At the end of most of the biographies she includes a section on whether each woman would consider herself a feminist. Not surprisingly, none of the women did. They attributed their success to individual skills and wartime opportunity. Williams consulted oral histories conducted during the 1980s when contemporary feminism was publicly under attack. The interviewers asked about pay equity, perceived competence of women, and work environment. Sears, Hopper, and Rees responded with the individualistic attitude typical of exceptional women active when organized feminism had little social support—if they could do it, anyone could.³

However, feminism per se is not gender. Barbara Stein follows a path similar to that of Williams. Her story of the exceptional Annie Montague Alexander argues that Alexander's family wealth allowed her to transcend most gender norms of the late nineteenth- and early twentieth-century United States. Feminism, or as Stein defines it, workplace discrimination, was not an issue to Alexander because she lived life "on her own terms" (xiv).

Like Williams's descriptions of World War II technology, Stein's expertise in evolutionary biology comes through clearly when she places the details of Alexander's life in the context of the developing science. Stein compellingly describes the contribution of Joseph Grinnell, the evolutionary biologist whom Alexander chose to direct U.C. Berkeley's Museum of Vertebrate Zoology. She highlights the unusual collaboration between Alexander and Grinnell as "an unusual synergy. . . . Their shared goals and complementary strengths allowed each to concede areas of expertise each to the other. In some arenas the two collaborated as equals, for example, fieldwork. In other spheres, growth and development of the museum's programs was left to the individual better qualified to carry out that portion of their plan: fiscal management [Alexander] of the museum versus research [Grinnell]" (88).

Stein began her research enchanted with Alexander—an independent, wealthy lesbian with a creative and skilled partner who chose to promote evolutionary biology as the focus of her life. Stein worked for more than a decade in the museum whose research agenda Alexander had shaped for forty years. The collective memory of "Miss Alexander" thrived in the musty catacombs of U.C. Berkeley's Museum of Vertebrate Zoology. Perhaps because of this, Stein's narrative makes Alexander the heroic excep-

³ Sears was interviewed in 1989, Hopper in 1982, and Rees in 1983–84. See Susan Ware (*Still Missing: Amelia Earhart and the Search for Modern Feminism* [New York: Norton, 1993]) for a discussion of individualism and feminism in the 1930s and 1940s.

tion to all gender and class stereotypes in the late nineteenth- and early twentieth-century American west.

Like Williams, Stein simplifies and overstates gender. In some instances, she is disturbingly wrong. She portrays Alexander's education in the 1880s at a two-year women's college as inherently "radical" and highly unusual for any woman (14). Yet the last quarter of the nineteenth century saw a rise in opportunities for women's higher education and the founding of prestigious and intellectually rigorous women's colleges such as Vassar and Wellesley. As well, she argues that Alexander's participation in any science, especially one that involved fieldwork, was almost unheard of for women. Yet at the same time Alexander collected mammals in California, several other women scientists in the San Francisco Bay Area continued the collecting and classifying they had done for at least four decades.

Despite these errors, Stein's biography of Annie Montague Alexander is fascinating. Stein, the vertebrate zoologist, was fortunate to write about a subject who, in Stein's words, "carried on a voluminous correspondence" (xv). "She might write as many as a dozen letters each day" (226). Thus, the strength of Stein's narrative reflects the sources she used—Alexander's "voluminous correspondence" shapes the roughly chronological narrative. In this light, it is understandable that the biography lacks depth about gender. The letters are so enthralling that the narrative kept my attention.

Williams's *Improbable Warriors* and Stein's *On Her Own Terms* point to the need for historians of American women and science to push themselves to use gender as an analytical tool. The work of historians of European women, notably Londa Schiebinger's on gender constructions in early modern European science, provides excellent models.⁴

⁴ Londa Schiebinger, *Nature's Body: Gender in the Making of Modern Science* (Boston: Beacon Press, 1993).

United States and International Notes

Signs: Journal of Women in Culture and Society welcomes announcements of fellowships, calls for papers, upcoming special issues, and new journals for the "United States and International Notes" section.

Announcements and calls for papers

The Gender and Diversities Institute at Education Development Center, Inc. (EDC) announces its first digital library initiative, the Gender and Science Digital Library project (GSDL). The GSDL will be developed in collaboration with the Eisenhower National Clearinghouse at The Ohio State University and is funded by the National Science Foundation. Please visit the Web site at <http://www.edc.org/GDI/GSDL> for information about the GSDL project. Application forms for material submissions and reviewer volunteers can be submitted on-line. The primary objective of the GSDL is to create a high-quality, interactive library of K-12, higher education, women's studies, and teacher preparation resources for science, technology, engineering, and mathematics (STEM) disciplines. It will assist educators and researchers in promoting and implementing gender-equitable STEM education in formal and informal settings, to both male and female students, and assist in increasing female involvement in the sciences. It will also provide resources to researchers and others working to understand the link between gender and science, including how gender influences the development of science and the role of women within science. Contact Sarita Nair, Project Director, Gender and Science Digital Library, Gender and Diversities Institute, EDC, 55 Chapel Street, Newton, MA 02458; telephone 617-618-2164; e-mail snair@edc.org.

Scholars are invited to contribute entries for a forthcoming two-volume reference set titled *Encyclopedia of Women's Autobiography* to be published by Greenwood Press in 2005. Please contact the press at editorial@greenwood.com for the list of people and subjects that will form entries to the encyclopedia. If you would like to participate in this project, please let the press know immediately, by e-mail. Details on the format of the entry, as well as a sample entry, will be mailed to you. There is a small monetary award and a complementary copy of the two-volume encyclopedia, at the time of its publication, for entries of more than 4,000 words. For entries of fewer than 4,000 words there will be a complementary copy. Complete manuscripts are due by May 1, 2003.

Feminist Theory announces an upcoming special issue on feminist theory and/or science, to be guest edited by Susan M. Squier. Articles are invited that consider the relations between feminist theory and science, as well as feminist theories of science. Essays may vary in subject area and methodology. Literary, historical, and/

or visual and cultural studies approaches; sociological and anthropological approaches; and perspectives from the scientific disciplines are encouraged. Possible subjects of exploration include feminist theory and the biological body and brain; the limits of materiality; the limits of social construction; and feminist theories of information and communication technology. Manuscript length should be between 6,000 and 8,000 words. Detailed notes for contributors are available on request from the *Feminist Theory* office: e-mail feminist.theory@york.ac.uk. Other inquiries should be directed to the issue editor by e-mail, at sxs62@psu.edu. Manuscripts should be clearly marked "Special Issue" and sent either to *Feminist Theory*, Centre for Women's Studies, University of York, Heslington, York YO10 5DD, United Kingdom, or, for North American authors, to Susan Squier, P.O. Box 557, 211 Miller Lane, Boalsburg, PA 16827. Deadline is December 15, 2003.

FEMSPEC, an interdisciplinary feminist journal dedicated to critical and creative works in the realms of science fiction, fantasy, magical realism, myth, folklore, and other supernatural genres, is accepting submissions for a special issue on black women's speculative fiction. This special issue will offer a range of critical approaches to black women's speculative fiction, film, other art forms, and black feminist theory. Critical essays will either take on a black feminist/womanist analysis or provide a strong gender critique. Possible topics may engage, although are not limited to, matriarchies and patriarchies of the future; black feminist (re)visions of the world; black witches; obeah women, root workers, and conjure women; gender dynamics; representations of black womanhood; rethinking (her)story through fantasy; and black women in Caribbean, African-American, or African folklore. Possible essays might question what it means for black women to create speculative visions of the world; how do speculative creations draw on black women's actual positions in the "real" world as women who experience varying degrees of sexism, racism, classism, and homophobia; how do current black women speculate, (re)write, (re)visit, and (re)envision history in ways that connect them to black women's legacies of struggle; why now—when black women have always emphasized speculation in their creative works—is there a surge of interest in their works; and why, as we move forward into the twenty-first century, is there a surge in fantastic representations of black womanhood? For further information or queries, please contact the special issue guest editors: Yolanda Hood, e-mail yhood@unca.edu, and Gwendolyn D. Pough, e-mail pough002@umn.edu. Submissions marked "Speculative Black Women: Magic, Fantasy, and the Supernatural" should be sent to *FEMSPEC*, Caddo Gap Press, 3145 Geary Boulevard, PMB 275, San Francisco, CA 94118. Please see the Web site at <http://www.csuohio.edu/femspec>. Deadline is July 30, 2003.

The *Journal of the Association for Research on Mothering (ARM)* seeks articles for an issue on mothering and teaching in the academy. The *Journal* welcomes submissions from students, activists, scholars, artists, and others who work or research in this area. Creative reflections such as poetry, short stories, and artwork on the

subject are also welcomed. Contact ARM, 726 Atkinson College, York University, 4700 Keele Street, Toronto, ON, Canada, M3J 1P3; telephone 416-736-2100, x60366; e-mail arm@yorku.ca. Visit the Web site at <http://www.yorku.ca/crm>. Deadline is May 1, 2003.

Call for artwork

Signs: Journal of Women in Culture and Society seeks submissions for cover art. Published quarterly by the University of Chicago Press and distributed internationally, *Signs* is an interdisciplinary academic journal that focuses on issues of gender, race, class, nation, and sexuality. Submissions are not limited by style or medium (photography and film stills are welcome) but should reproduce well in black and white; content should represent a point of view on women's issues. One full-color cover will be published annually. Send up to ten labeled slide duplicates to Art Editor, *Signs*, University of California, Los Angeles, 1400H Public Policy Building, Box 957122, Los Angeles, CA 90095-7122. E-mail signs@signs.ucla.edu. A small honorarium is available. Deadline is ongoing.

About the Contributors

Lots W. Banner is professor of history and gender studies at the University of Southern California. She is the author of numerous articles and books in the history of women and gender, including *American Beauty* (New York: Knopf, 1983); *In Full Flower* (New York: Knopf, 1992); *Finding Fran* (New York: Columbia University Press, 1988); and the forthcoming *Intertwined Lives: Margaret Mead, Ruth Benedict, and Their Circle* (New York: Knopf, 2003). She is a past president of the American Studies Association, the Conference Group in Women's History, and the American Historical Association, Pacific Coast Branch.

Karen Barad is professor of women's studies and philosophy and chair of women's studies at Mount Holyoke College. She is also affiliated with the critical social thought program. Her Ph.D. is in theoretical particle physics. Her research in physics and philosophy has been supported by the National Science Foundation, the Ford Foundation, the Hughes Foundation, the Mellon Foundation, and the National Endowment for the Humanities. She is the author of numerous articles on physics, philosophy of science, cultural studies of science, and feminist theory. Her forthcoming book is titled "Meeting the Universe Halfway."

Nancy W. Brickhouse is professor and associate director of the School of Education at the University of Delaware. She is also editor of the journal *Science Education*. She has published numerous articles on gender, multiculturalism, and science education, including "Embodying Science: A Feminist Perspective on Learning," *Journal of Research in Science Teaching* 38 (2001): 282–95, and, with William Stanley, "Teaching Sciences: The Multicultural Question Revisited," *Science Education* 85 (2001): 35–49.

Arny Bug received her Ph.D. in physics from MIT in 1984, has been in the Department of Physics and Astronomy of Swarthmore College since 1988, and is currently the department chair. She teaches across the standard physics curriculum and also teaches a seminar on gender and physical science. She does physics research in partnership with Swarthmore undergraduates. Current projects involve computer simulations of positron annihilation in materials. She is the mother of two sons, Murphy, twelve, and Moes, six.

Margaret W. Conkey (conkey@esd.berkeley.edu) is the Class of 1960 Professor of Anthropology at the University of California, Berkeley, where she also is director of the Archaeological Research Facility. She has served as chair of the Association for Feminist Anthropology of the American Anthropological Association. She publishes on aspects of early human visual culture and on feminist archaeology. She

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Patricia Adair Gowaty is professor of ecology at the University of Georgia. She is an evolutionary biologist interested in the ecology of social behavior.

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Ruth Hubbard is professor emerita of biology at Harvard University. In her laboratory research she elucidated the way light interacts with the visual pigment molecules in the retina of the eye, whose transformations initiate the sensation of vision. Since the mid-1970s she has mainly focused on the interrelationships of science and society, especially as they affect women as both scientists and as objects of scientific enquiry and technical manipulation. She is a cofounder of the Council for Responsible Genetics and currently serves on its advisory and editorial boards. Her publications include *The Politics of Women's Biology* (New Brunswick, N.J.: Rutgers University Press, 1990); *Exploding the Gene Myth* (coauthored with Elijah Wald; Boston: Beacon Press, 1993, 1998); and *Profitable Promises* (Monroe, Me.: Common Courage Press, 1994).

Pamela S. Lottero-Perdue is a doctoral student in curriculum and instruction at the University of Delaware. She has a bachelor's degree in mechanical engineering, a master's in education, and a physics teaching certificate. She was a process engineer and, more recently, a teacher of both students and teachers in physics and pre-engineering. In addition, she has written curricula for Project Lead-the-Way, a national pre-engineering organization. From these experiences, she has developed a strong interest in issues of gender within science and engineering. She coauthored, with Nancy Brickhouse, "Learning on the Job: The Acquisition of Scientific Competence," *Science Education* 86, no. 6 (2000): 756-82.

Shella McManus is currently assistant professor in the history department at the University of Winnipeg. Her research and teaching interests include the North American west in the nineteenth century, Canadian-American relations, and the history of sexuality. Her most recent article, "Mapping the Alberta-Montana Borderlands: Race, Ethnicity, and Gender in the Late Nineteenth Century," was published in the *Journal of American Ethnic History* 2, no. 3 (Spring 2001): 71-87.

Patricia Moore is assistant professor of history, women's studies, and environmental studies at Green Mountain College in Vermont, a liberal arts college with an environmental focus. Her research areas include gender and science, U.S. women's history, and U.S. environmental history. She is currently investigating how ideas about nature and gender were expressed in public policy in the early twentieth-century United States. She has also written about the interplay between gender and expressions of scientific authority in the development of botany as a scientific discipline.

Sue V. Rosser is dean of Ivan Allen College, the liberal arts college of Georgia Institute of Technology in Atlanta, where she is also professor in the School of History, Technology, and Society. She holds a Ph.D. in zoology from the University of Wisconsin—Madison and served as a director of women's studies for more than twenty years in Virginia, South Carolina, and Florida, as well as senior program officer for women's programs at the National Science Foundation. Author of eight books, she has published more than ninety-five journal articles on the theoretical and applied aspects of women and science and women's health. Her current book, *The Story of Academic Women Scientists*, is under contract with Routledge.

Londa Schiebinger is Edwin E. Sparks Professor of History of Science and codirector of science, medicine, and technology in culture at Pennsylvania State University. She is author of *The Mind Has No Sex? Women in the Origins of Modern Science* (Cambridge, Mass.: Harvard University Press, 1989); the prizewinning *Nature's Body: Gender in the Making of Modern Science* (Boston: Beacon Press, 1993); *Has Feminism Changed Science?* (Cambridge, Mass.: Harvard University Press, 1999); editor of *Feminism and the Body* (Oxford: Oxford University Press, 2000); coeditor, with Angela Creager and Elizabeth Lunbeck, of *Feminism in Twentieth-Century Science, Technology, and Medicine* (Chicago: University of Chicago Press, 2001); and a section editor of the *Oxford Companion to the Body* (Oxford: Oxford University Press, 2001). Her current research explores gender, commerce, and politics in the European voyages of scientific discovery.

Joni Seeger is professor of geography and women's studies at the University of Vermont. She is the author of several books, including *The State of Women in the World Atlas* (New York: Penguin, 1997, 2003) and *Earth Politics: Coming to Feminist Terms with the Global Environmental Crisis* (New York: Routledge, 1993). She is active in the Committee on Women, Population, and Environment, a coalition of feminist activists and academics working on environmental issues.

Bonnie B. Spanier (spanier@albany.edu) is associate professor at the University at Albany in the women's studies department. She holds a microbiology doctorate and was a cofounder of the Capital Region Action Against Breast Cancer (CRAAB!)

and the National Breast Cancer Coalition's Project LEAD. She has written *Im/partial Science: Gender Ideology in Molecular Biology* (Bloomington: Indiana University Press, 1995) as well as analyses of biological determinist claims about homosexuality. A consultant on curriculum change for equity in higher education, particularly in the sciences, she has taught feminist thought and public policy; women, biology, and health; and women's issues in science.

Banu Subramaniam (banu@wost.umass.edu) is assistant professor of women's studies at the University of Massachusetts, Amherst. She is coeditor of *Feminist Science Studies: A New Generation* (New York: Routledge, 2001). She works at the intersection of gender, race, and science and is interested in engaging the feminist studies of science in the practice of science.

Lisa H. Weasel is assistant professor of biology at Portland State University in Oregon. She is coeditor of *Feminist Science Studies: A New Generation* (New York: Routledge, 2001) and is currently principal investigator for the National Science Foundation-funded project The Global Helix: Ethics and Equity Issues in International Biotechnology, which investigates ethics and equity issues relating to biotechnology in the United States, India, South Africa, and Switzerland.

Christine Wertheim (c_wertheim@hotmail.com) is on the faculty of the School of Critical Studies at the California Institute of the Arts. She writes on subjectivity, space, and gender. She recently cofounded the Los Angeles-based Institute for Figuring.

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Anna Wilson (a.m.Wilson@bham.ac.uk) is lecturer in American studies at the University of Birmingham, England. She is the author most recently of *Persuasive Fictions: Feminist Narrative and Critical Myth* (Cranberry, N.J.: Associated University Presses, 2001), a reappraisal of the effectiveness of feminist cultural interventions. Her current research focuses on homophobic violence in the heartland and on how accounts of this violence are used in the construction of American national narratives and the building of public consensus.

Mary Wyrer (mary_wyrer@ncsu.edu) is assistant professor in women's and gender studies and multidisciplinary studies at North Carolina State University. Her research focuses on the processes, practices, and outcomes of integrating women's studies content into the science and engineering curriculum. Her publications include "Women and Computer Technologies," special issue of *IEEE Technology*

and Society Magazine 18, no. 4 (Winter 1999/2000), coedited with Alison Adam; "The Science and Politics of the Search for Sex Differences," special issue of *NWSA Journal* 12, no. 3 (Fall 2000), coedited with Laura Severin; and *Women, Science and Technology*, coedited with Mary Barbercheck, Donna Cookmeyer, Hatice Ozturk, and Marta Wayne (New York: Routledge, 2001). She recently won a National Science Foundation ADVANCE Leadership Award for her efforts.

Guidelines for Contributors

The editors invite submission of article-length manuscripts that might appropriately be published in *Signs: Journal of Women in Culture and Society*. We publish articles from a wide range of disciplines in a variety of voices—articles engaging gender, race, culture, class, nation, and/or sexuality. We are looking for lively, provocative essays that launch new inquiries or prompt intense debate; we publish essays not only in areas of scholarship familiar to *Signs* readers but in newly emergent fields relevant to women and gender as well. Essays may be discipline specific if they are cross-disciplinary in their theorizing, their methodology, or their sources.

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Submissions should follow the author-date system of documentation, with limited endnotes, as outlined in *The Chicago Manual of Style* (14th ed.). (See chapter 16 outlining documentation for the social sciences.) The journal office may request full revision of manuscripts not meeting the CMS requirements for documentation.

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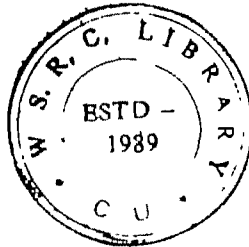
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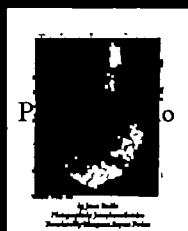
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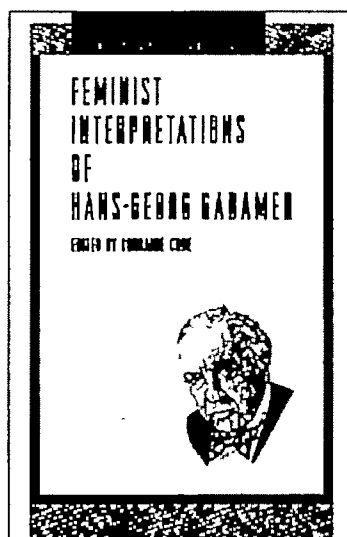
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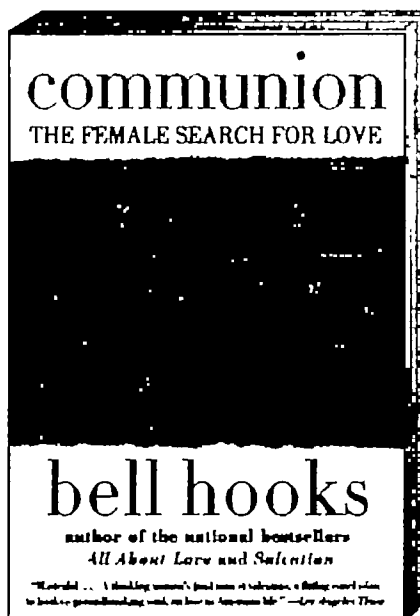
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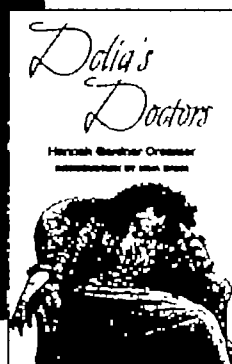
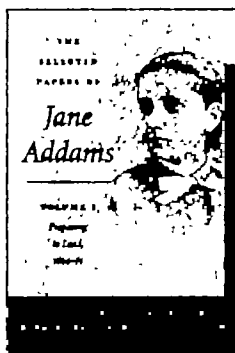
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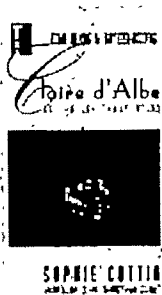
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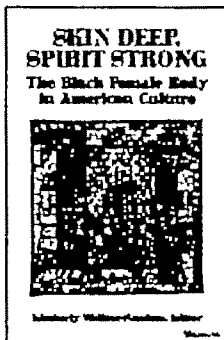
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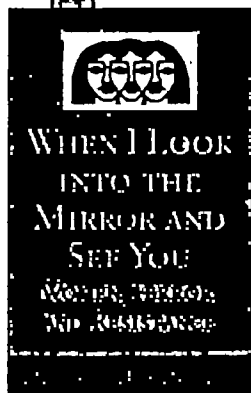
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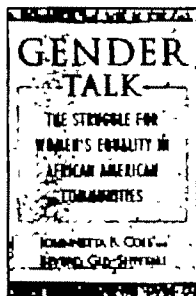
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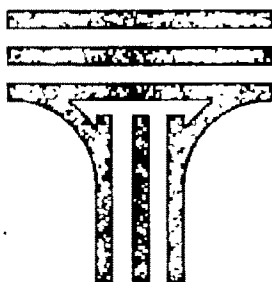
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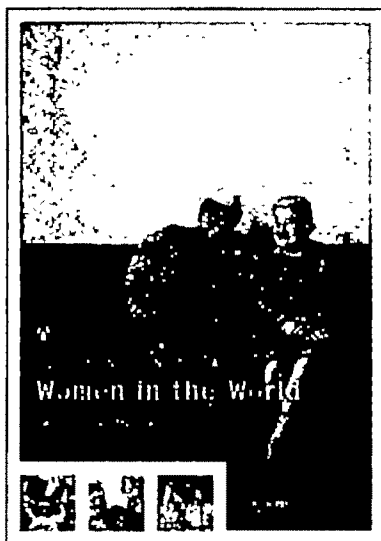
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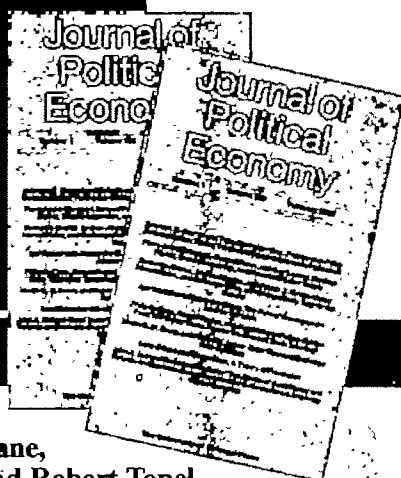
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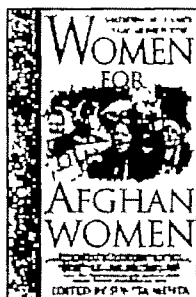
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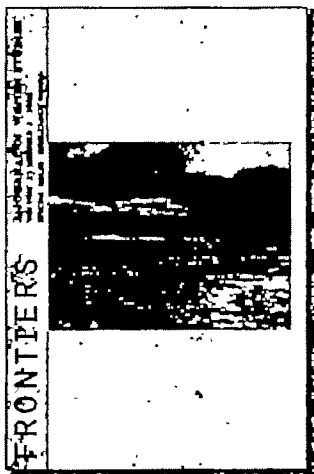
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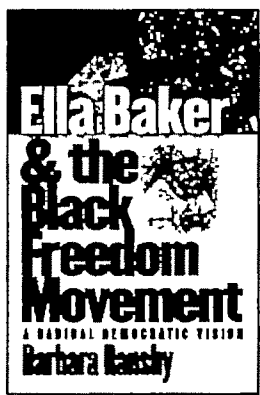
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